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SCIENCE

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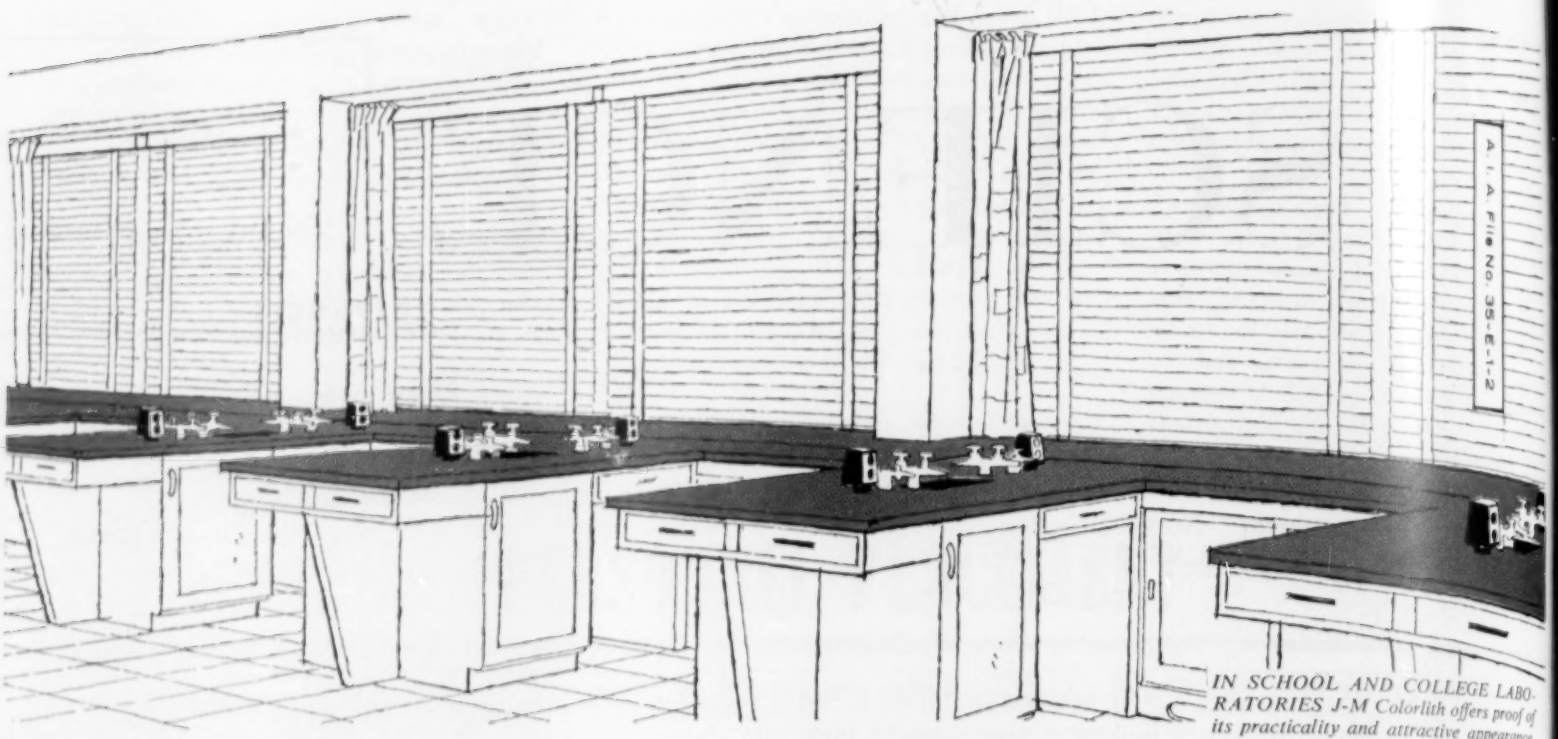

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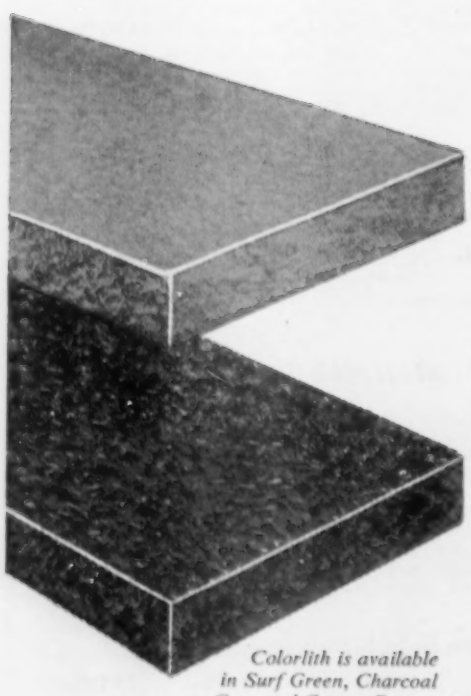

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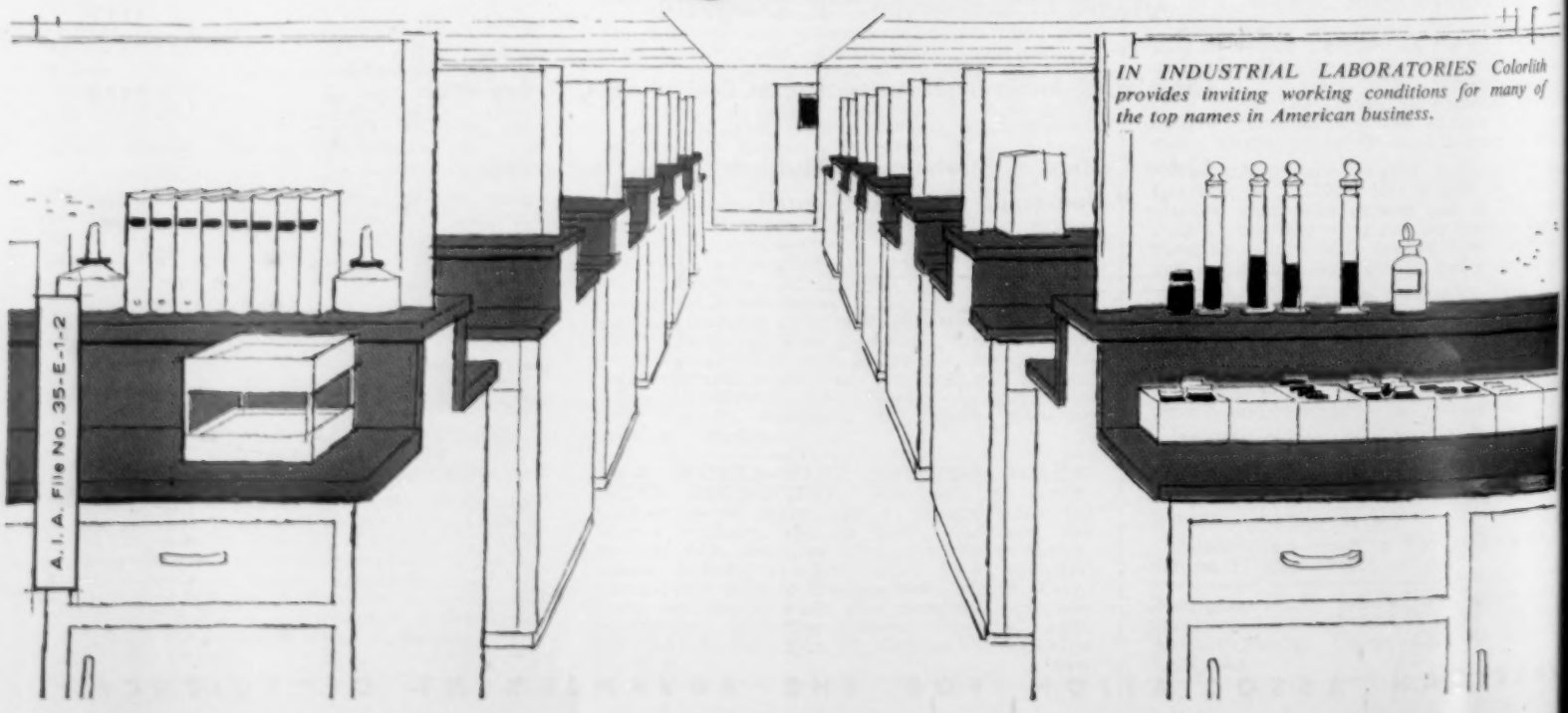

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No Spouting Fish

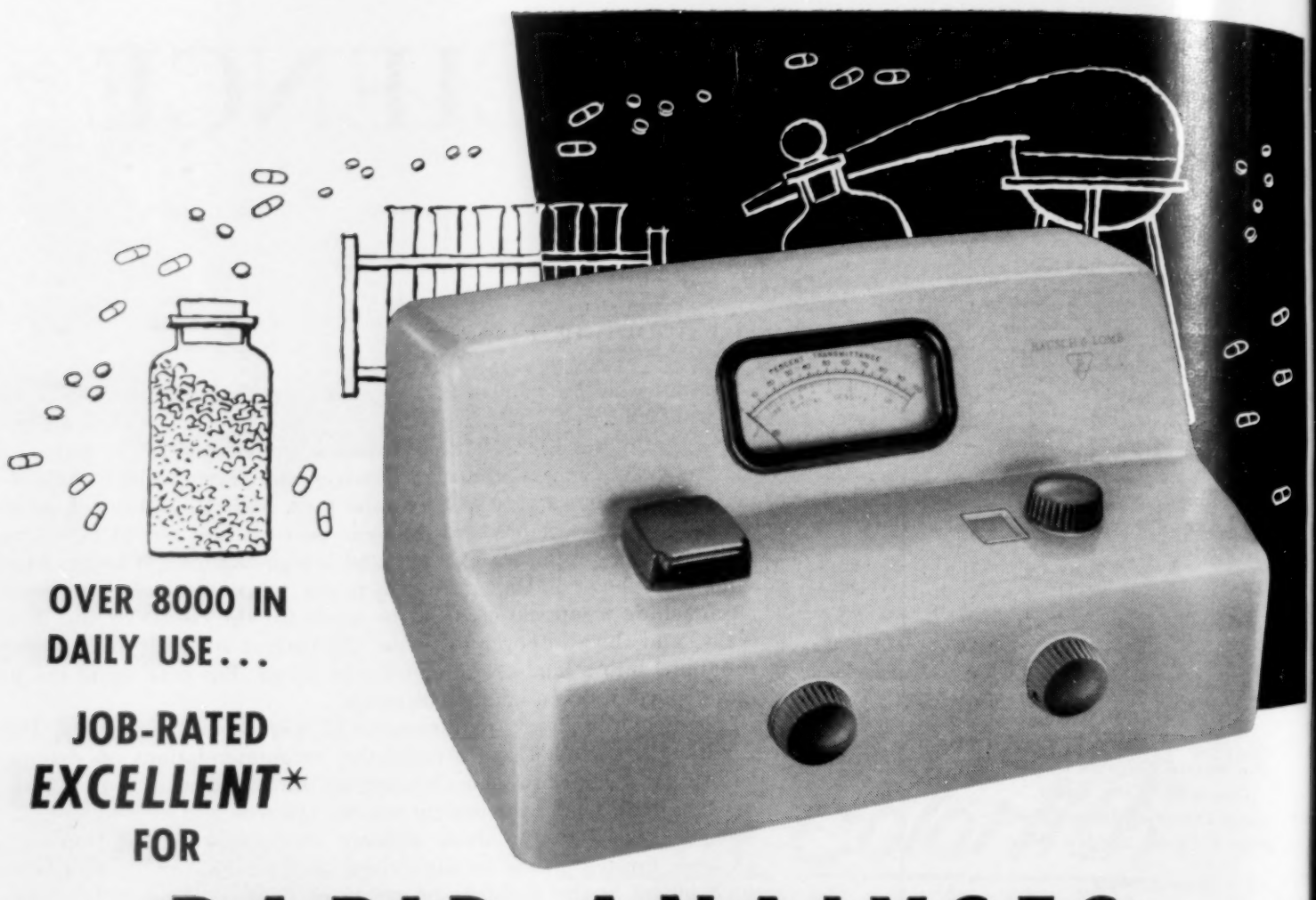
Perhaps the last man to present an effective case for calling a whale a fish was Herman Melville. In *Moby Dick* he observes that, for the purposes of his messmates from Nantucket, it was quite sufficient to put whales in the same class with shad, sharks, alewives, and herring—with the difference that whales spout and have horizontal tails. Melville lost out, however, for not all men are whalers and different purposes are served by taking note of such other properties as lungs and a four-chambered heart. Much the same story holds in the study of a newer species of leviathan. Something approaching a rational case can be made for the efforts by the Army, the Navy, and the Air Force to define the various research and development programs into a land-sea-air scheme of things. But here again the press of larger purposes is in another direction.

To meet the future requirements of science and weaponry, President Eisenhower included in his legislative recommendations on defense reorganization several provisions bearing on the administration of research and development. Under the present system, research and development are conducted largely by the three military services, with the Department of Defense limited chiefly to supervisory and review responsibilities through such offices as the Assistant Secretary of Defense for Research and Engineering. A step in the direction of the proposed changes was taken recently, however, when the Advanced Research Projects Agency was set up in the Department of Defense, with the authority to initiate its own projects in its own facilities.

The Administration's bill, which was sent to Congress 16 April, would authorize the appointment of a Director of Defense Research and Engineering whose supervisory and review duties would be supplemented by the power to direct those "research and engineering activities that the Secretary of Defense deems to require centralized management." The salary of the director would be equal to that of the Secretaries of the military departments. According to the President's special message to Congress of 3 April, the new position would be established in place of the present Assistant Secretary of Defense for Research and Engineering.

The case against introducing major changes in the administration of research and development, insofar as it is rational, is based largely on the claim that an increase in centralization implies a decrease in the opportunity for giving fresh ideas a hearing. If competition among the services is replaced by central management, so the argument runs, then not only will duplication of effort be eliminated but also that which is being duplicated. New approaches will be controlled out of existence. This argument, when pushed, also calls for changes, but in the opposite direction. In the May issue of *Fortune*, Burton Klein of the Rand Corporation finds that the responsibility for selecting projects should rest with the services and that the present review apparatus in the Department of Defense should be largely eliminated.

Many of the details of the President's plan have yet to be made explicit, including the arrangements with the existing research and development programs in the services and in the Department of Defense. And much will depend, as is so often the case, upon the precedents set by the first man to occupy the key office. We nevertheless see no reason why getting new ideas into circulation need necessarily be linked to an arbitrary system of categories. In the appointment of Herbert F. York as chief scientist of the Advanced Research Projects Agency, the Defense Department has shown that it can pick the right man for an important post. We hope that the President's research and development recommendations are enacted into law, and we trust that the Defense Department will find its Linnaeus.—J. T.



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SCIENCE

Evolution at Work

The pressing problems today center on the mechanisms of evolution and the biological uniqueness of man.

Theodosius Dobzhansky

When hunger and other elemental needs are satisfied, people are apt to ask questions about human nature, about man's origins, and about his place in the scheme of things. Some people ask such questions even when hungry and suffering. It would be naive to claim that an evolutionary approach supplies all the answers, but it is relevant to these questions and may profitably be used as a guiding light in the quest for some of the answers. Accordingly, the two short papers by Darwin and by Wallace, read before the Linnean Society of London in 1858, mark a watershed in the intellectual history of mankind. These papers contained the essentials of the theory of biological evolution. They did not explicitly deal with man; but, in 1871, Darwin showed that man is a part of nature and a product of the evolutionary process.

The theory of evolution has not only become a focus of biology but has influenced human thought in much wider domains. Many people who are not biologists are at least dimly aware of this. By way of illustration, permit me to recount some reminiscences. A few years ago, as I stepped ashore from a small launch in a village on one of the tributaries of the Amazon River, I was met by a man who proved to be the local agronomist. His first question was, what influence might Lysenko's discoveries (of the spurious nature of which he was, of course, unaware) have on our ideas about evolution. In Egypt, a friend translated for me parts of a book recently written by a Coptic hermit who lived for many years in one of the desert monasteries. The book contained a very fair exposition of evolutionism, followed by a refutation on what, to the author, seemed sufficient theological grounds. Punta Arenas claims to be the southernmost city in the world; the region of the Straits of Magellan where it is located is sometimes described as the "Uttermost Part of the Earth." A Chilean friend and I were asked to give public lectures on evolution in the hall of the Punta Arenas City Library. We complied, and found that a part of the audience was not unfamiliar with the topic.

Dr. Dobzhansky is professor of zoology at Columbia University. This article is adapted from a paper presented 27 Dec. 1957 during the Indianapolis meeting of the AAAS.

Historical Background

The idea of evolution in the broadest sense of universal and all-pervading change and development is with many of us a habit of thought. We take it for granted because in our lifetimes we have seen so many innovations—telephones and radios, automobiles and airplanes, plastics and antibiotics, atomic bombs and artificial satellites. Things were not always changing so fast. Lucretius, one of the most lucid thinkers of antiquity, was able to write that "all things remain the same even if you should outlast all the ages in living; and still more would you see them the same if you should never come to die."

Christianity is implicitly evolutionistic; it posits a historical process which moves from the Creation to the Fall, the Redemption, the City of God. However, it took some fourteen centuries to make it explicitly evolutionistic—from Saint Augustine in the 5th century to Vico in 1725, Condorcet in 1793, Darwin and Wallace in 1858, 1859, and 1871, and Marx in 1859 and 1867. Condorcet held that the history of mankind was a gradual but steady ascent from a primitive savagery to ever higher states: man is bound to reach perfection in a not too distant future. This cheerful view might sound almost too smug did we not know that it was written while its author awaited execution as a counter-revolutionary.

The idea of progressive evolution in human affairs reached the acme of popularity during the Victorian era. Civilization was supposed to bring ever more material and spiritual comforts, very quickly to some but, in the long run, to almost everybody. Those who were receiving the comforts readily believed that this admirable prospect would be realized most expeditiously through private enterprise and free competition. Marx recommended rather different methods, which he believed to be somehow deducible from Darwin's discoveries. He proposed to acknowledge his indebtedness by dedicating *Das Kapital* to Darwin—an honor which Darwin politely declined. Marxism is sometimes dubbed a Christian heresy; it promises a socialist City of God but is curiously vague about just what this blessed state will be like.

The favorable intellectual climate of the last century speeded up the acceptance of the discoveries of Darwin and Wallace. In turn, biological evolutionism exerted ever-widening influences on the natural and social sciences, as well as on philosophy and even on politics. Not all of these extrabiological repercussions were either sound or commendable. Suffice it to mention the so-called social Darwinism (*1*), which often sought to justify the inhumanity of man to man, and the biological racism which furnished a fraudulent scientific sanction for the atrocities committed in Hitler's Germany and elsewhere. But these are

merely perversions of Darwinism. In the words of Paul Sears (*2*), "Charles Darwin did not kill the faith of mankind. He wrought mightily, and others with him, for a newer and greater faith—faith in universal order, whose secrets open themselves to men truly free to question, to communicate, and to arrive at agreement as to what they have seen."

One problem took precedence in biology during the latter part of the 19th and the early years of the current century. This was the validation of the evolutionary interpretations of the facts of zoology, botany, and anthropology. I have no wish to dogmatize, but this problem appears to have been definitively settled. The occurrence of the evolution of life in the history of the earth is established about as well as events not witnessed by human observers can be. The evidence has not satisfied quite everybody; a few people who are not ignorant of the pertinent facts are nevertheless antievolutionists. However, biological research directed towards producing more evidence that evolution has taken place is no longer urgent.

Guessing where new discoveries are likely to be made is a risky venture in science. And yet, a scientist is constantly forced to take this risk; the success of his work depends on the perspicuity of his guesses. With this reservation, it may be said that the most pressing problems of evolutionary biology seem at present to belong to two groups—those concerned with the mechanisms of evolution and those dealing with the biological uniqueness of man.

Factors of Evolution

Darwin did not eschew making hypotheses concerning the forces which bring evolution about. Without a plausible explanation of how evolution might happen it would be hard to accept the idea that it did happen. The theory of natural selection was Darwin's answer, and from the vantage point of modern knowledge it can be seen that the answer was substantially correct. But Darwin was fully aware that, given the state of biology in his day, a causal analysis of the evolutionary process was unattainable. A number of discoveries in our time made possible at least a start in this direction. The chief one was the discovery and the rediscovery of Mendel's laws; then came the unraveling of the chromosome behavior in cell division, fertilization, and meiosis; the finding of mutations by de Vries and their study by Morgan and his school; the induction of mutations by x-rays and other agents, first revealed by Muller; and the foundation of population genetics by Hardy, Weinberg, and Chetverikov.

The dates of most of these discoveries fall between 1900 and 1930. Strange to say, it was during this period that some biologists professed much skepticism about the feasibility of explaining evolution in terms of the processes then being discovered. Bateson, one of the leaders of genetics in its formative years, was the foremost skeptic. This attitude is still lingering in some places, especially in continental Europe. New and unassimilated information has evidently acted like the proverbial trees which hid the forest.

A most creative phase of modern evolutionism opened around 1930. Perhaps for the first time in the history of biology, the leading roles in the development of a field passed to theoreticians using the tools of mathematical analysis, and their analysis far outdistanced the observational and experimental work. Fisher, Wright, and Haldane developed, almost simultaneously, a mathematical theory of Mendelian populations. The fundamental component of evolutionary changes was perceived to be the alteration of the frequencies of genic and chromosomal variants in living populations. Mutation, natural and artificial selection, random drift, and gene diffusion between populations are the agents known to bring about such alterations. These are, then, the causative factors of evolution.

The logical step towards a satisfactory theory of evolution should now be to study quantitatively the factors of evolution and their interactions in free-living, domesticated, and experimental populations. This is an exciting but difficult task; so great is the complexity of most evolutionary patterns that precise measurement is rarely attainable. Determination of the orders of magnitude of some of the forces may, however, be within the range of what is possible; even such rough approximations will shed needed light on the mechanisms of evolution.

Natural Selection and Balanced Polymorphism

It is not my purpose here to review the field of quantitative studies on the factors of evolution. I choose rather to consider some illustrative examples.

For many years natural selection was something which biologists frequently discussed but seldom did anything about. To Darwin, natural selection was an inference from a mass of indirect evidence; he argued that it should occur, but he did not claim to have directly observed natural selection acting to produce changes in free-living populations. This he could not do because the selective advantages and disadvantages which slowly change natural populations are mostly too small to be readily detectable. To be sure, one can observe elimination of victims of heritable malformations and diseases. Natural selection is, accordingly, often compared to a sieve, which lets some particles pass but sequesters others. Such a process can prevent the accumulation of hereditary diseases and consequent degeneration of a species. It is less easy to see how it may lead to adaptive improvements.

Studies on microorganisms have changed the situation considerably. In 1943, Luria and Delbruck (*3*) analyzed the origin of bacterial strains resistant to destruction by bacteriophages, and their type of analysis was rapidly extended to explain the origin of bacterial resistance to antibiotics and similar phenomena. Mutants which confer upon the bacteria their resistance to phages, or to antibiotics, arise from time to time in most or in all cultures. However, such mutants are too rare to be noticed unless a selective or screening agent is applied. When a suspension of phages is added to a bacterial culture, all the bacteria except for the few phage-resistant mutants are killed; when an antibiotic is added, only the resistant mutants survive. Ingenious methods have been devised for estimating how often the resistant mutants arise. For example, the frequency of the mutation for the phage resistance in the colon bacteria, *Escherichia coli*, is of the order of 10^{-7} to 10^{-8} per cell generation.

The selection of resistant mutants in bacteria is a process which resembles the sieve in the above analogy too closely to be a good model of the selective processes in higher, sexually reproducing, organisms, including man. Materials more suitable for the study of these processes have been found. Fisher (*4*) showed in 1930 that if the heterozygote for two genetic variants, A_1A_2, is superior in fitness to both corresponding homozygotes, A_1A_1 and A_2A_2, the natural selection will, in an outbreeding sexual population, act to maintain both A_1 and A_2 with frequencies that may readily be computed. Some twenty years ago, Ford (*5*) discovered this situation, known as

balanced polymorphism, in nature in some butterflies. More recent studies show that balanced polymorphism is more frequent than was formerly suspected. Natural populations of the flies *Drosophila* have yielded some beautifully clear examples. Moreover, the selective pressures acting on some polymorphic natural populations are, as will be shown below, astonishingly great. This is a boon to the experimental evolutionist, for natural selection becomes at last observable and its magnitude measurable.

Here we may digress to consider the possibility that balanced polymorphism may occur in human populations. The problem is of more than academic interest, since balanced polymorphism has a property which is at first sight astonishing. Provided that the heterozygous carriers of hereditary defects or diseases are superior in fitness to the noncarriers, natural selection will maintain these defects in the populations. The work of Allison (*6*) on the sickle-cell anemia, and that of Ceppellini on the Mediterranean anemia, have yielded at least presumptive evidence of balanced polymorphism. The homozygotes for the respective mutant genes usually die of severe anemias, but the heterozygotes may not only be healthy but, at least under certain conditions, may be relatively immune to some malarial fevers as compared with the normal homozygotes. Carter, Penrose, and Wallace (*7*), among others, have considered the possibility that many genetic variants in man which are deleterious when homozygous may be beneficial when heterozygous. This possibility has often been studiously ignored or dismissed on insufficient grounds. It would greatly complicate several important issues, among them that of the genetic effects of atomic radiations on human and other populations. However, it is becoming evident that the oversimplified models of the genetic population structure are proving inadequate if not positively misleading. Since I recently had an opportunity to discuss this matter in *Science* (*8*), I leave it here with the remark that the need for a better understanding of the genetic processes taking place in living populations is now felt more keenly than ever.

Natural Selection in Experimental Populations of Drosophila

The trait which proved to be highly favorable for experimental studies on evolution is a cryptic one. Many natural populations of most species of *Drosophila* are polymorphic for variations in the structure of certain chromosomes, due to so-called inversions of blocks of genes. These variants of the chromosome structure are inherited as simply as are the genes that determine the blood groups for which human populations are polymorphic. A further similarity is that the flies which carry different chromosomal types are externally as indistinguishable as are people with different blood groups. The chromosomal types may, however, be diagnosed easily and precisely in stained preparations of the salivary glands of the fly larvae. Every race or population of a given species of *Drosophila* may be characterized in terms of the relative frequencies of the different chromosomal types which it contains (*9*), just as human populations can be described in terms of the relative frequencies of the different blood group genes.

But here the analogy ends, since the chromosomal types which a *Drosophila* carries may easily be shown to influence its fitness, while the problem of the functional significance of the blood groups in man is still full of uncertainties. The experiments with *Drosophila* are arranged as follows. We collect a sample of the population in some natural locality where the flies occur; place the females singly in laboratory culture bottles and allow them to produce progenies; examine the chromosomes in these progenies and pick out the strains which carry the desired chromosome types; and make up a mixture of flies carrying certain chromosome types in known proportions. This mixture is placed in specially constructed population cages in which the flies will breed freely for as many generations as the experimenter may allow. These populations are kept under controlled conditions which can be varied at will, and at desired time intervals we take samples of eggs which the flies in the cages deposit and investigate the chromosomes in the larvae which grow from these eggs.

Such experiments show that the fly which is fittest in most environments usually turns out to be a heterozygote. A fly in which the two chromosomes of a pair differ in structure, say A_1A_2, enjoys hybrid vigor, heterosis, as compared with the homozygotes, A_1A_1 and A_2A_2. There is every reason to think that this heterosis occurs in the environments in which the flies live in nature as well as in the laboratory. The chromosomal polymorphism is balanced polymorphism. Furthermore, at least some of the chromosomal heterozygotes are favored by amazingly powerful selective forces. The magnitude of the selection can be estimated from the speed with which the frequencies of the different chromosomal types undergo changes in the experimental populations, and from the equilibrium proportions that are eventually reached. Thus, in a certain experiment with *Drosophila pseudoobscura*, the following situation was observed: Taking the fitness of a heterozygote, A_1A_2, to be unity, the fitnesses of the homozygotes, A_1A_1 and A_2A_2, proved to be 0.90 and 0.41, respectively (*9*).

Consider the meaning of these figures. The adaptive value of the homozygote A_2A_2 is less than one-half of that of the heterotic type, A_1A_2. Taking the heterozygote as the standard of fitness, the homozygote, A_2A_2, having less than 50 percent of the standard fitness, must technically be classed as a semilethal. Or one may say that the homozygote A_2A_2 is afflicted with a hereditary disease, or a constitutional weakness. Now, this would not greatly surprise us if A_2 were a mutant obtained in the laboratory, under the influence of, say, x-ray treatments. But A_2 is a permanent component of many flourishing populations of *Drosophila* in nature. The A_2A_2 homozygotes are not laboratory artifacts: they occur abundantly in nature.

Seasonal Genetic Changes in the Make-up of Drosophila Populations

Not enough is known about the adaptive functions which the chromosomal polymorphism performs in nature. Quite possibly these functions are different in different species of *Drosophila*. Observations on populations of *Drosophila pseudoobscura* in some parts of California furnish a clue for this species. In these populations, the relative frequencies of different chromosomal types change with the seasons; some chromosomes are more common in spring than in summer or in fall, while other chromosomes show the reverse seasonal trends (Fig. 1). *Drosophila* produces in nature several generations per year—we do not know just how many. At any rate, natural selection is so intense that the populations undergo genetic reconstructions which fit them to seasonal changes in their environments. Here, then, are evolutionary changes, microevolutionary ones to be sure, which are observable directly in nature in a free-living animal species.

Further light on these evolutionary changes comes from laboratory experiments. The seasonal genetic changes indicate that the adaptive values of the chromosomal types vary in different environments. The carriers of some of the chromosomes are relatively fitter in spring and those of others are superior in summer or in fall. Experiments bear this out; the adaptive values of the chromosomal types are exquisitely sensitive to environmental modification. The series of adaptive values of three chromosomal types cited above (1:0.90:0.41) was observed in experimental populations kept at 25°C. Lowering the temperature by 9°, to 16°C, makes the adaptive values uniform, or so nearly so that no differences can be detected in our experiments within the limits of resolution. The genotype which causes a hereditary infirmity at 25° is completely "cured" at 16°C. This emphasizes how meaningless may be the distinctions between "superior" and "inferior" hereditary endowments if the environment is not specified.

The seasonal genetic changes in *Drosophila pseudoobscura,* observed in nature in the population of Piñon Flats, Mount San Jacinto, California, have been reproduced rather fully in experiments (*9*). In nature, a certain chromosome type increases in frequency at the expense of another type between March and June, the changes are reversed between June and September, and the frequencies remain static from September to March. The kind of genetic changes which occur in nature during the summer months have been easily imitated in experimental population cages kept at 25°C. The winter stability is reproduced if the same population cages are kept at 16°C. All attempts to duplicate the spring situation in population cages were unsuccessful. The experiments of Birch (*10*) showed why this should be so; the changes which occur in nature during spring can be copied experimentally only if the fly larvae do not live in crowded conditions (as they always do in population cages).

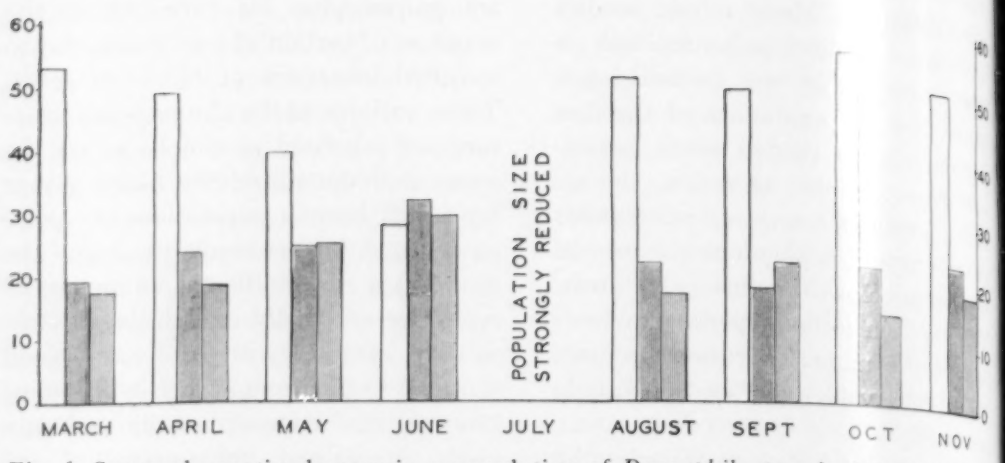

Fig. 1. Seasonal genetic changes in a population of *Drosophila pseudoobscura* inhabiting a certain locality in California (Piñon Flats on Mount San Jacinto). The heights of the columns indicate the average percentages of three different chromosomal types in different months in samples taken from 1939 to 1954. (Data of Dobzhansky, Epling, *et al.*)

Alteration of Drosophila Populations in California (1940–1957)

The seasonal genetic changes observed in nature in *Drosophila pseudoobscura* are evolutionary changes by definition. However, because of their cyclic character, the alterations induced at one season are reversed at the next season. The biological significance of the chromosomal polymorphism lies evidenty in that it confers a marvelous adaptive plasticity upon the populations. The populations are able to respond by adaptive genetic changes to temporary, and even to seasonal, shifts in their environments. This is, of course, a kind of evolutionary luxury which only a rapidly breeding animal, like *Drosophila,* is able to afford.

The genetic plasticity also permits, however, rapid modifications in response to more lasting alterations in the environment. This creates an opportunity for the direct observation of these evolutionary changes in nature. Changes in the relative frequencies of chromosomal types lasting for several years have been recorded in some populations of *Drosophila pseudoobscura.* Some of these changes may have been caused by succession of droughty and wet years, but this is not established securely. Recently it was discovered that still another, and apparently more enduring, change is going on in certain populations of the same species.

Reference has been made above to the fact that populations or races of a *Drosophila* species may be described in terms of relative frequencies of different types of chromosomes in their chromosome pools. Such a description was made in 1944 for *Drosophila pseudoobscura,* on the basis of samples of the populations of this species collected in western United States and in Mexico, chiefly during the period 1938 to 1940. This study showed that a chromosome type, denoted as PP, is the dominant form (occurring in more than 50 percent of the chromosomes) in Texas and also along the eastern face of the Rocky Mountains. The PP chromosomes wane in frequency as one proceeds westward. Among the approximately 20,000 chromosomes scored from populations of California, only four PP chromosomes were found, in three different localities. This is a very low frequency, 0.02 percent (Fig. 2).

The first intimation that the populations were changing came in 1946 and 1947, when the population of Mather, in the Sierra Nevada of California, was found to contain about 0.5 percent of PP chromosomes. None were found there in 1945. However, in 1950 the frequency of PP stood at 2.8 percent, in 1951 at 4.5 percent, in 1954 at 11.1 percent, and in 1957 at 10.0 percent. Similar changes took place on Mount San Jacinto, where C. Epling found the first PP chromosome in 1951. By 1955 the frequency had risen to 7.7 percent.

In an attempt to elucidate the nature of these changes, in the summer of 1957 I sampled the populations of ten localities in California and of ten in Arizona and Utah (*11*). More or less adequate population samples had been taken in or near all these localities in 1940, 1941, or earlier. The striking fact which this study has revealed is that, between 1940 and 1957, the PP chromosomes have become fairly common in every one of the California populations sampled. Their frequencies now range between 5.0 and 12.0 percent (Fig. 3). Furthermore, the waxing of PP chromosomes has taken place chiefly at the expense of another chromosome type, denoted CH, the frequencies of which have markedly waned in most California populations.

In contrast to the genetic upheaval in the California populations, no spectacular changes were found in Arizona and Utah. In 1940 as well as in 1957, some PP chromosomes (fewer than were found in California in 1957 but more than in 1940) and some CH chromosomes (fewer than in California) occurred in the

populations ... Arizona and Utah. This is important since a conjecture which had to be excluded was that the sharp rise of PP in California might have been due to a westward migration of the eastern (Texan) populations, in which PP chromosomes are predominant.

The rise of PP chromosomes in California represents a more impressive evolutionary change than appears at first sight. The average frequency of PP in California populations was close to 0.02 percent in 1940 and 8 percent in 1957. This is a 400-fold increase. The estimated mean number of fly generations in natural habitats over a period of 17 years is probably of the order of 100 (more than twice this number could be obtained in the laboratory). A 400-fold increase in the frequency of a genetic variant in 100 generations bespeaks a quite considerable magnitude of the adaptive advantage, and hence of natural selection.

In fact, the only comparable evolutionary change ever observed in free-living animals is the development of the so-called industrial melanism in England and in some localities on the continent of Europe. Dark variants, due to single dominant mutant genes, appeared in several species of moths approximately one century ago. Now these variants have become frequent in populations of localities in which the vegetation is polluted by industrial fumes. This has been brought about by the action of natural selection, since the dark variants appear to be protectively colored on polluted, and the light ones on unpolluted, vegetation (*11*). The spread of the melanic variants in moths is thus caused by human interference (industrial pollution) with the habitats of certain free-living species. The cause which has brought about the rise of PP chromosomes in the California *Drosophila pseudoobscura* is, unfortunately, unknown. There is, however, some circumstantial evidence that this cause is not man-made. If this is so, the genetic alterations in these *Drosophila* populations represent the greatest observed effect of natural selection in an animal species not appreciably influenced by man.

Microevolution, Mesoevolution, and Macroevolution

It is needless to labor the point that the evolutionary changes described above are small compared to those which led from the eohippus to the modern horse, or from an australopithecine-like animal to man. The former are microevolutionary and the latter macroevolutionary changes. Nevertheless, microevolution and macroevolution are parts of a single continuum, and studies on the former help to elucidate the latter. After all, the knowledge of the atomic fission and fusion reactions gained in laboratories helps in understanding the evolution of stellar systems, although even the biggest hydrogen bombs generate amounts of energy which are puny compared to those produced in the sun or in stars. This argument is not meant to imply that studies on macroevolution may be dispensed with. The evidence of paleontology, while not completely clear and consistent, is in favor of the view that macroevolution is compounded of mi-

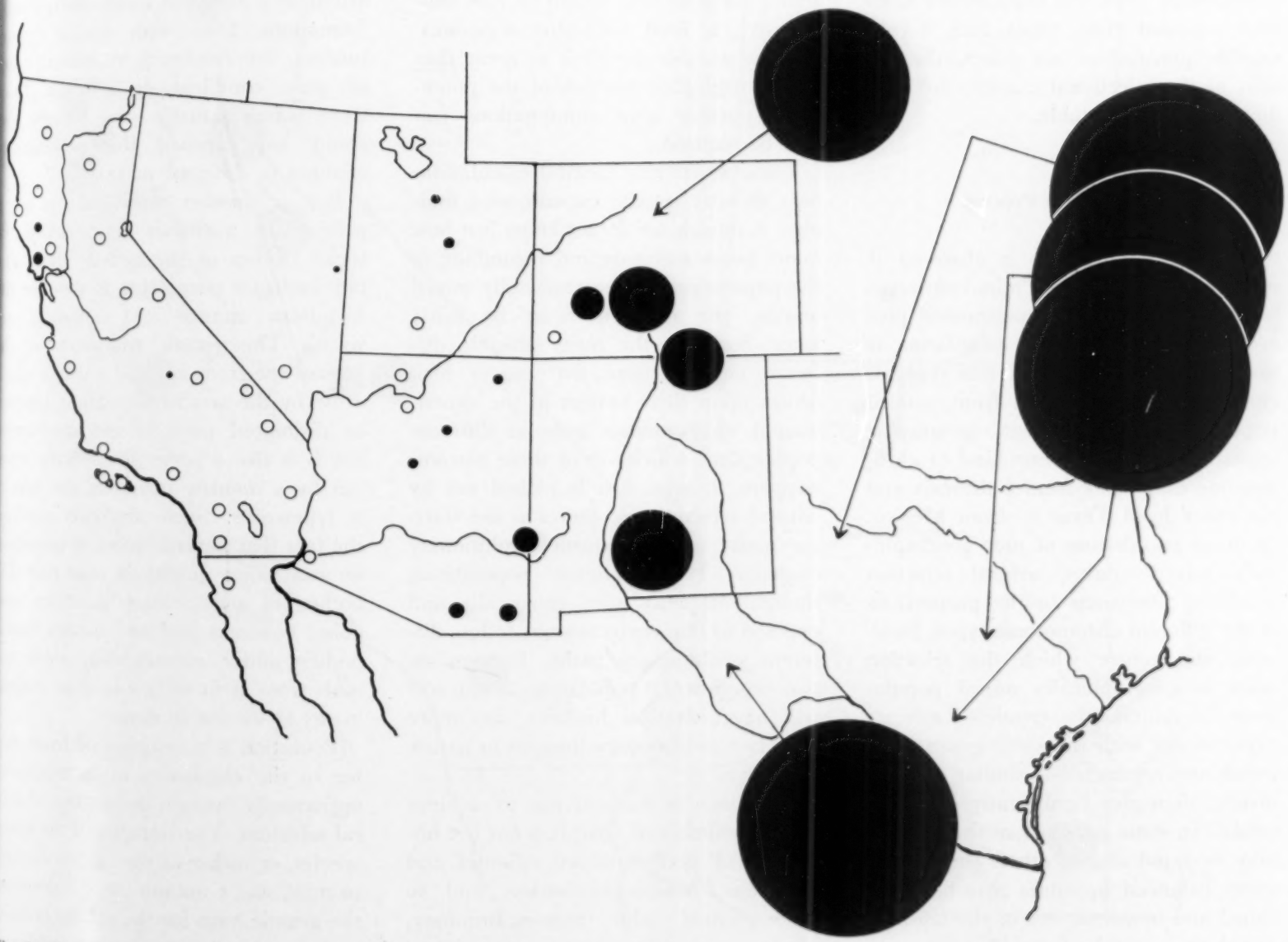

Fig. 2. The status of populations of *Drosophila pseudoobscura* in the southwestern United States according to samplings made chiefly in 1940 and earlier. The diameters of the black circles are proportional to the frequencies of a certain type of chromosome (PP) in the populations of different localities. Open circles indicate populations in which this type of chromosome was not encountered.

croevolutionary events. The problem of macroevolution is, then, essentially that of the patterns of microevolutionary events which yield macroevolutionary changes of different kinds.

This problem is beyond the confines of the present discussion: macroevolution cannot be observed at work; only the end-products of its action on our time level can be studied. However, we have recently succeeded in producing in experiments some genetic changes which seem to transcend the limits of microevolution, and for which I have suggested a tentative label of "mesoevolution" (*13*).

Reference has already been made to natural selection in experimental populations of *Drosophila*. A mixture of flies with chromosomes of different types, but derived from a natural population of the same geographic locality, is introduced into a population cage; the proportions of these chromosomal types may change from generation to generation, until equilibrium frequencies are attained. The position of the equilibrium depends upon the environment in which the population is kept. The changes observed are microevolutionary ones; the experiments have been repeated many times, and, if reasonable precautions are taken, the results of the selectional changes are predictable and repeatable.

Evolution as a Creative Process

Now, something else is observed if what may appear to be a minor variation is introduced into the experimental procedure. An experimental population is made up in which the different types of chromosomes are derived from natural populations of different geographic regions; for example, one kind of chromosome may come from California and the other from Texas or from Mexico. In many populations of such geographically mixed origins, natural selection produces alterations in the proportions of the different chromosome types. However, the course which the selection takes in geographically mixed populations is remarkably erratic. Replicate experiments, with the same genetic materials and conducted in similar environments, often give significantly divergent results. In some populations the changes may be rapid and in others sluggish; in some, balanced equilibria may be established and in others, one of the chromosomal types may be lost (*13*).

This may seem to be a strange and even disconcerting situation. Is it not the criterion of validity of a scientific experiment that its results should be reproducible? Yet in these experiments we face a real biological indeterminacy, and this fact is fraught with implications. Evolutionists, particularly those who work with fossils, long ago pointed out that the evolutionary transformations which occur in a group of organisms are unrepeatable and irreversible. The macroevolutionary changes represent unique and nonrecurrent evolutionary histories. Notwithstanding many instances of parallel or convergent evolution, we have no reason to think that any form of life has arisen two or more times independently.

The experiments on *Drosophila* populations of geographically mixed origin throw some light on this situation. The key to the problem lies in the prodigious, and indeed prodigal, efficiency of sexual reproduction in the creation of novel genetic endowments. It is easy to show that with n genes each represented by m variants (alleles), the number of potentially possible gene combinations is m^n. An estimate of 1000 for the number of genes (n) and of 10 for the number of alleles per gene (m) would be very conservative, at least for higher organisms. But the number 10^{1000} is so great that only a negligible fraction of the potentially possible gene combinations can ever be realized.

These apparently fanciful calculations bear directly on the experimental findings. Although we do not know just how many genes segregate and recombine in the populations of geographically mixed origins, the numbers must be fairly large. Some of the many possible different gene patterns that confer high fitness upon their bearers in the experimental environments arise in different populations; whichever of these patterns happens to arise first is picked out by natural selection and serves as the starting point of subsequent evolutionary changes. The replicate populations, though originally alike genetically and exposed to like environments, follow different evolutionary paths. Perhaps no two experimental populations of this sort will have identical histories, any more than two evolutionary lineages in nature will have.

Evolution is not striving to achieve some foreordained goal; it is not the unfolding of predetermined episodes and situations. Macroevolutionary, and to some extent also mesoevolutionary, changes are unique, nonrecurrent, and creative. It is necessary to make quite clear what is meant by creativity of biological evolution. This is a creative phenomenon because evolution brings about novel and harmonious genetic equipments which enable their carriers to survive in some environments. These genetic equipments are mostly new combinations of genes. But the process of formation of new gene combinations is not of the kind to which one can apply the French saying that "the more it changes the more it remains the same thing." Organic development is not gradual accretion of traits produced by the genes independently of each other; the adaptive value of a genetic equipment is a function of all the genes which in the organism are acting in concert.

Man as a Product of Evolution

Man was not programmed in biological evolution, because evolution has no program. In one sense, man, *Drosophila*, and all other forms of life are evolutionary accidents. If slightly different environmental opportunities had been offered to their far and near ancestors, quite different creatures might have arisen as a result of evolutionary transformations. Even with similar opportunities, the formation at critical times of gene combinations different from those which actually were formed also could have turned the evolutionary changes to different paths.

But, in another sense, man is not a product of a chance concatenation of lucky throws of the genetic dice. The old analogies purporting to describe the fortuitous nature of evolution are wrong. The genetic equipment of the human species is not like a watch which arose by the accidental coming together of disjointed parts of the mechanism, nor is it like a poem accidentally typed out by a monkey pounding the keys of a typewriter. Such analogies overlook the fact that natural selection introduces an antichance quality in evolution. The bodies of our animal ancestors were going concerns and not merely human bodies under construction; these animals were as fit to live in their environments as we are in ours.

Evolution is a response of living matter to the challenges of environmental opportunity through the process of natural selection. The response of the human species, or rather of the species ancestral to man, was a unique one—it developed the genetic basis for the accumulation of and for the extragenic transmission of a body of learned tradition called culture. The relations between culture and

its genetic basis are all too often misunderstood. This topic is too complex and important to be dealt with lightly, but the basic facts are simple enough. Genes determine the possibility of culture but not its content, just as they determine the possibility of human speech but not what is spoken. The cultural evolution of mankind is superimposed on its biological evolution; the causes of the former are nonbiological without being contrary to biology, just as biological phenomena differ from those of inanimate nature but are not isolated from them (*14*).

Human Evolution at Work

The genetic equipment of our species was molded by natural selection; it conferred upon our ancestors the capacity to develop language and culture. This capacity was decisive in the biological success of man as a species; it enabled man to acquire unprecedented powers to change and control his environment at will. The very success of culture as a nonbiological adaptive instrument means, however, that man has crossed the Rubicon—he has become specialized to live in man-made environments.

Some strange conclusions are sometimes drawn from the above facts. One is that human biological evolution has ended and has been replaced by evolution of culture. Another is that all men are uniform in their genetic equipment, at least insofar as the latter conditions the capacity to undergo socialization and acculturation. Another is that man's "intrinsic" intelligence (whatever that may mean) has not changed since the times of the Cro-Magnon, or even of the Java man. Still another is that natural selection no longer operates in modern mankind, since men live in such hopelessly unnatural environments.

All these notions overlook the simple fact that it is precisely because the capacity to create, absorb, and transmit culture is so decisive in the success of man as a species that natural selection works not only to preserve but also to augment this capacity. Human biological and cultural evolutions are not separated in watertight compartments. They are interacting processes. All men are equal in rights, but they are most certainly not biologically uniform. Our genetic diversity does influence our tastes and aptitudes for different occupations and professions. But this does not make some of us superior and others inferior; no human being should ever be used as a means to an end.

All human societies, the civilized even

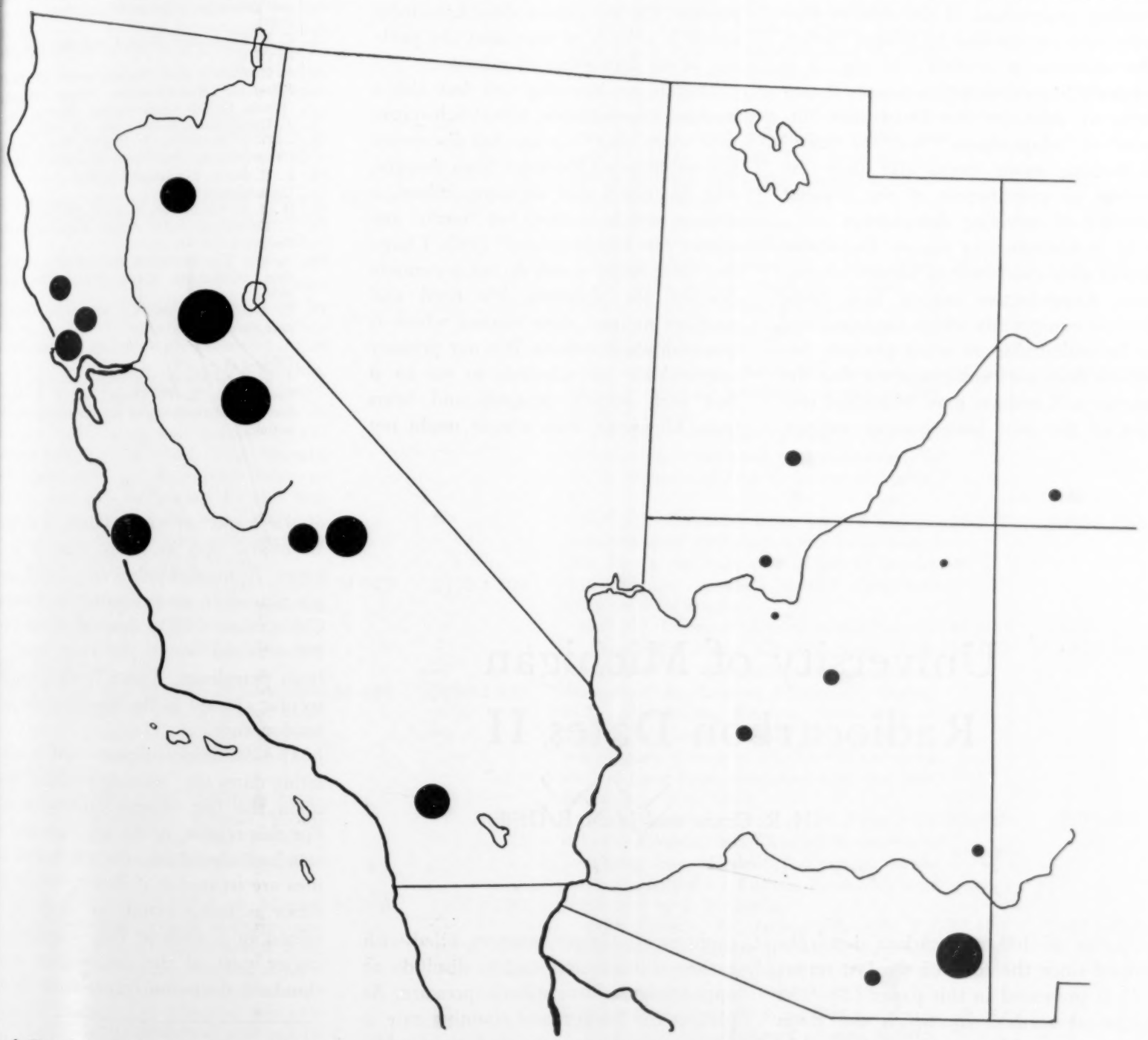

Fig. 3. The status of populations of *Drosophila pseudoobscura* in the southwestern United States in 1957. The diameters of the black circles are proportional to the frequencies of a certain type of chromosome (PP). Although the scale of the map is larger than that of the map in Fig. 2, the scale of the black circles is the same in both figures.

more than the primitive ones, have numerous vocations to be filled. Natural selection has made all healthy human beings trainable for the performance of diverse duties. This is, then, a biological adaptation which makes people multiform, not uniform as is sometimes supposed. Educability, the ability to be trained, is consistently fostered in man by natural selection. And yet, the carriers of certain specialized genetic equipments, such as musicians or poets, may excel in the performance of some specialized functions.

Natural selection is active in all human societies, including the most advanced ones (*15*). It must be understood that there is nothing esoteric about the "naturalness" of natural selection. All that "selection" means is that the carriers of different genetic equipments contribute unequally to the gene pool of the succeeding generations. If the relative contributions are decided by human choice, the selection is artificial. If not, it is natural. Natural selection usually maintains or enhances the Darwinian "fitness" or "adaptedness." But "the fittest" is nothing more spectacular than the parent or grandparent of the greatest number of surviving descendants.

It is erroneous to equate Darwinian fitness with excellence in human estimation. Reproductive success may favor genetic equipments which we may hold to be undesirable on other grounds. Selection does not even guarantee that the species will endure; most biological species of the past have become extinct, without issue, and yet their evolution was controlled by natural selection. This is because selection promotes what is immediately useful, even if the change may be fatal in the long run.

The biological evolution of our species continues to be at work. Perhaps no other problem of science is more challenging than the understanding of the biological and cultural evolutions of mankind in their interactions. As pointed out above, evolution in general has no program, and the evolution of man is no exception. No biological law can be relied upon to insure that our species will continue to prosper, or indeed that it will continue to exist. However, man is the sole product of evolution who knows that he has evolved and who has continued to evolve. It is up to man to supply the program for his evolutionary developments which nature has failed to provide. He has gained some knowledge which is a basis of hope that the problem is not impossible of solution.

This is an inspiring task but also a crushing responsibility. Albert Schweitzer once wrote that "our age has discovered how to divorce knowledge from thought, with the result that we have, indeed, a science which is free, but hardly any science left which reflects" (*16*). I hope that these angry words do not accurately describe the situation. We need and we have at least some science which is free and which reflects. It is our primary responsibility as scientists to see to it that such science prospers and bears fruit. Moreover, such science ought not to be a monopoly of some kind of technological elite. People at large, and particularly men of action who make the decisions which control so much in our lives, need not be as woefully ignorant of even the simplest principles of science as they are. At least some of the ideas which guide our work as scientists are not beyond the understanding of people of average intelligence who are not scientists professionally. The idea of evolution is one of them. As expounded by Darwin, it is one hundred years old, but we have barely begun to understand its full consequences (*17*).

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17. I wish to thank my colleagues Drs. J. A. Beardmore, L. C. Dunn, and J. A. Moore for critical readings of the manuscript of this article.

University of Michigan Radiocarbon Dates II

H. R. Crane and James B. Griffin

A list of 109 radiocarbon dates obtained since the time of the last report (*1*) is presented in this paper (*2*). The technical method by which the dates were measured has not been changed in any essential way. Two complete counter systems are in continuous operation. The counters are Geiger counters, filled with carbon dioxide and carbon disulfide at approximately atmospheric pressure. At present the background counting rate is 6.5 counts per minute, and eight additional counts per minute are obtained from carbon of zero age. The duration of the count on each sample is at least 48 hours, and in many cases it is 72 hours. Approximately every fourth sample placed in each counter is of known C^{14} content: CO_2 derived either from 200-year-old wood (by ring count) or from petroleum. There is no detectable secular change in the results of the calibration runs.

The calibration figures used in calculating dates are "moving averages" based upon the last several calibration runs. For this reason, in the calculation of the standard deviation, the calibration figures are treated as if they contained four times as many counts as would be obtained in a 48-hour run. Therefore the major part of the contribution to the standard deviation comes from the run

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on the unknown sample. The standard deviation calculated purely from the numbers of counts on the unknown and on the calibration samples forms a useful minimum figure. The standard deviations for various ages, calculated on the assumption that the run on the unknown is 48 hours and that those on the calibration samples are four times as long, are as follows: for samples of zero age, ±81 years; for samples of age 5600 years, ±131 years; for samples of age 11,200 years, ±235 years; for samples of age 16,800 years, ±453 years; for samples of age 22,400 years, ±890 years.

In the date list given in Table 1, the standard deviations given will be found to be greater than those given above, generally by about a factor of two. We know that there are sources of uncertainty in addition to that contributed by the counting statistics, for example, the chemical process, the condition of the original sample, variations in the operation of the counters, and so forth. In each case the excess in the standard deviation above that given in the table represents our best estimate about the additional uncertainties attendant on the measurement of the particular sample. In a few cases there were specific circumstances which could affect the reliability of the measurements; for example, a lack of sufficient material to fill the counter, or the presence of visible rootlets which were not completely removable. In such cases a notation is included with the description of the sample.

Opinions about the reliability, plausibility, or interpretation of dates are to be found in some of the descriptive paragraphs. These are to be attributed to the persons who submitted the samples for dating.

Table 1. Radiocarbon dates.

Description	Sample No.	Age (yr)
I. *Upper Mississippi Valley*		
Sorg Site (21DK1), Minn. Charcoal from a limestone hearth found 19 in. below the surface in excavation unit 1. Cultural materials in the zone from 16 to 36 in. below the surface are in the Sorg focus, unnamed aspect. This is a Middle Woodland period focus with pottery in the Hopewellian tradition. Site excavated by the St. Paul Science Museum; sample submitted by Louis H. Powell.	M-447	800 ± 200
Kolterman Mound 18, Dodge County, Wis. Effigy Mound culture. Charcoal from cremation in heart region of otter effigy mound. Associations: *Madison Cord* impressed type pottery vessel and two chipped-stone implements. Submitted by Warren L. Wittry, State Historical Society of Wisconsin (*3*).	M-398	1180 ± 250
Modoc Rock Shelter, Randolph County, Ill. This site has previously been dated, and human occupation ranging back to 7922 B.C. ±392 years was indicated (*4*). Samples collected in 1953. They should yield dates later in time and refer to cultural materials belonging to the late Archaic occupations of the site. Collected by Melvin L. Fowler and submitted by Thorne Deuel, Illinois State Museum.		
Charcoal from 7½-ft level in square 35:0. Eight feet above the top samples collected by Matson (C-899 and C-900) which were dated at 5955 ± 235 and 5268 ± 230 yr (*5*).	M-483	4720 ± 300
Charcoal from 9½ to 10½ ft in square 35R5. This sample is 5½ ft above Matson's highest samples (C-899 and C-900) which were dated at 5955 ± 235 and 5268 ± 230 yr (*5*).	M-484	5280 ± 300
Clayton County, Iowa. Samples excavated from Sny-Magill Mound group (*6*), lots 1 and 2, sec. 23, T. 94 N., R. 3 W. Submitted by Paul Beaubien, National Park Service.		
Charcoal from east portion of Mound No. 43. This mound, conical in shape, approximately 78 ft in diameter and 6 ft in height, contained bundle burials, copper beads, "Red Ocher" blades, and several layers of red ocher. The charcoal was collected from a partly consumed pole not in close association with the principal inclusions, but it must have been in place when the mound was formed. Sherds of a later-day pottery type, related to *Madison Cord Impressed,* found in the mound fill closer to the surface than the charcoal and away from the central portion of the mound, apparently were introduced after the original period of mound construction.	M-305	2430 ± 250
Charcoal from Mound No. 24. The small sample for analysis was assembled by collecting very small pieces just below the surface of the mound. The charcoal was not associated with the mound's intentional inclusions. This recent date is not acceptable to determine the age of the mound's primary construction.	M-306	430 ± 200
Charcoal from Mound 27, a bird effigy, found as scattered finds in mound fill from 6 to 18 in. below the surface; could be recent. Collected and submitted by Paul L. Beaubien.	M-307	< 200
Charcoal from the west portion of Mound 43 which produced sample M-305 as given above.	M-308	2500 ± 250
Steuben Mound group, Marshall County, Ill. Collected and submitted by G. D. Morse and Dan F. Morse of Peoria, Ill.		
Ma°202. Charcoal from burned log associated with extended adult male burial No. 43 on the east side of pit D on the floor of the mound. Should date late Hopewell occupation.	M-378	1660 ± 250
Ma°202. Charred bone of burial No. 43.	M-380	1650 ± 250
Cromwell, Noble County, Ind. Tusk fragments of the Richmond mastodon. Submitted by Everett Burmaster, Irving, N.Y., and Irving Reiman, University of Michigan. Compare with sample M-138, which gave a date of 5300 ± 400 on wood said to have been associated with the tusk fragments (*1*, p. 667).	M-139	12,630 ± 1000
Dreckshage site, west of St. Peters, St. Charles County, Mo. Charred house beam overlying late Mississippi Trappist house floor. Collected by Eugene Kozlovich and submitted by J. B. Griffin.	M-323	530 ± 200
Pike County, Ill. Irving Site (Pk 2). Charcoal from square B. Should date late Hopewell or the Irving Late Woodland level. Collected and submitted by J. C. McGregor, University of Illinois.	M-489	1180 ± 250
Platte County, Missouri. Curtiss Mound of Keller-Brenner. Excavated and submitted by J. M. Shippee, University of Missouri.		

Description	Sample No.	Age (yr)
Charcoal from fire-burned area in lower levels of mound. Original number 2f.	M-399	1500 ± 250
Charcoal from deep in the mound and about 2 ft from the edge of the fire-burned area. Original number 3.	M-400	1650 ± 250
Mankato, Blue Earth County, Minn. Wood from the middle till which lies stratigraphically below the youngest till in the Mankato vicinity. The middle till is probably the blue or black pre-Wisconsin till mentioned by Leverett and the "Kansan" drift mentioned in a footnote of the Glacial Map of North America (*7*). Similar samples (W-300 and W-301) submitted to the U.S. Geological Survey laboratory have been dated at more than 35,000 years ago (*8*). Collected and submitted by James H. Zumberge, University of Michigan.	M-412	> 25,000
Raaf Shell Mound (12 Sp 1), Spencer County, Ind. Fresh-water mollusk shells from Archaic shell heap on the north bank of the Ohio River. Collected and submitted by Glenn A. Black, Indiana Historical Society, Newburgh.		
9 ft below surface.	M-196	6600 ± 400
8 ft below surface.	M-197	6150 ± 400
7 ft below surface.	M-198	5940 ± 400
6 ft below surface.	M-199	6170 ± 400
5 ft below surface.	M-200	6000 ± 350
4 ft below surface.	M-201	6250 ± 350
II. *Great Lakes*		
Lenawee County, Mich. American mastodon (*Mammut americanus*) palate (No. 29276 University of Michigan Museum of Paleontology) and tusks from beneath 2 ft 4 in. of muck, resting on a beaver-cut limb of aspen and blue-gray clay containing mollusks in sec. 5, T. 8 S., R. 2 E. Discovered during drainage of a bog area on the J. M. Bruggeman farm. Submitted by Claude Hibbard, University of Michigan.		
Inside portion of mastodon tusk.	M-280	7070 ± 240
Outside portion of mastodon tusk.	M-281	7820 ± 450
Jackson County, Mich. Wood from marl deposit 4 ft below surface and associated with Jefferson mammoth. Site is 11.5 mi southeast of Eaton Rapids. Submitted by R. H. Baker, Michigan State University.	M-507	12,200 ± 700
Peat from top of 30-in. layer of peat near South Haven. Dates the rise in water level from the Chippewa stage to the Nipissing stage (Lake Michigan basin) (Zumberge and Potzger, *9*).	M-291	4000 ± 300 4000 ± 350
Sanilac County, Mich. Submitted by J. H. Zumberge, University of Michigan.		
Hemlock log (10 in. in diam.), south side of Mill Creek, 3 mi south of Lexington, on the shore of Lake Huron. Log is imbedded in 1-ft gravel lens, underlain by 3 ft of laminated clay resting on hard gray till. The gravel layer lies below a few feet of bedded medium-to-fine sand, probably of Nipissing age. The contact of gravel on the underlying clay is uncomformable. The gravel probably represents the deposit of stream flowing into a post-Algonquin low-water stage in the Lake Huron basin prior to the Stanley level.	M-299	7270 ± 450
Wood from the north side of the mouth of Mill Creek on the shore of Lake Huron, 3 mi south of Lexington. This specimen comes from what appears to be the same gravel bed as sample M-299, although the stratigraphic relationships are less clear. The date is not compatible with that of M-299; hence some error is involved, probably in the field interpretation.	M-300	12,336 ± 700
III. *Northeastern United States*		
Snell site, St. Johnsville, Montgomery County, N.Y. Excavated in 1949 by a joint expedition of New York State Museum and Rochester Museum of Arts and Sciences. The site is one of early Owasco culture in eastern New York. Collected and submitted by W. A. Ritchie.		
Charcoal from pit 13. Date shows considerable variance with those of M-28 (*1*, p. 667) and M-492 (see below), yet all pits definitely pertained to the single period of occupation at the site. In comparison with other dated early Owasco sites in eastern New York (M-176, M-177) (*1*, p. 668), the plus value of this sample (A.D. 985) would appear to be the most accurate of all dates obtained for this site.	M-178	1170 ± 200
Charcoal from pit 20. This date seems too recent. However, the plus value of this sample agrees very closely with the minus value of sample M-178, which would appear to be the most accurate of all dates obtained for this site (see M-178 above).	M-492	800 ± 200
Bannerman site, Dutchess County, N.Y. Charcoal from hearth at 45 in. from the surface at the base of an implement-bearing level. This sandy stratum, 2 to 3 ft thick, underlay two undisturbed sterile strata. No pit lines could be detected about the hearth. The cultural materials found throughout the occupied zone are indicative of the Laurentian phase of the middle Archaic and include the ground slate ulo. The sample would seem to date an early Laurentian component in the mid-Hudson valley. Collected by W. A. Ritchie and James Shafer, submitted by W. A. Ritchie.	M-287	4480 ± 300
Castle Creek site, Broome County, N.Y. Charcoal from a pit containing burned stones and potsherds of Castle Creek styles, from a section excavated by the Broome County Historical Society. Late Owasco culture. This sample was sent as a check on sample M-179 (*1*, p. 668), with which it is in general agreement. These late dates tend to support the original interpretation of the site as showing Owasco-Iroquois temporal overlap and cultural interaction. Submitted by Foster Disinger, Binghamton, N.Y., through W. A. Ritchie.	M-493	760 ± 200
Orient #2 site, Suffolk County, N.Y. Charcoal from a fire kindled on the floor of a large burial pit of the Orient culture. This eastern Long Island culture, showing transitional features from late Archaic into early Woodland, is one of the northeastern cultures which participated in a complex, early mortuary ceremonialism. Collected by Roy Latham, Orient, N.Y., and submitted through W. A. Ritchie.	M-494	2900 ± 250
Stony Brook site, Suffolk County, N.Y. Charcoal from sec. W. 5 N. 15, from a dry sand stratum, 22 to 26 in. deep in midden. Orient culture habitation site. Collected and submitted by W. A. Ritchie.	M-587	2900 ± 250

Description	Sample No.	Age (yr)
Charcoal from pit 6, covering portions of sections E. 45 N. 30, 35, and 40, and E. 40 N. 30, 35, and 40. Collected from the lower level of a 5-ft deep pit. Orient culture habitation site. Collected by James V. Wright, submitted by W. A. Ritchie.	M-588	2930 ± 250
Sugar Loaf Hill, Suffolk County, N.Y. Charcoal taken from a depth of 2 to 2½ ft, in direct association with grave goods, from a burial pit of the Orient culture. Collected and submitted by W. A. Ritchie.	M-586	3000 ± 300
Athol, Mass. Poplar log from a road cut through a bog ¾ mi west of Pleasant Street along new Massachusetts Route No. 2. The cut showed 6 ft 3 in. of humified and fibrous peat overlying 2 ft 11 in. of gyttja which in turn lay directly on unweathered stratified sand. The log was imbedded in gyttja 7 ft 8 in. below the bog surface and 1 ft 6 in. above the stratified sand. Pollen analysis of the profile by Margaret Bryan Davis showed the log to be associated with a zone in which spruce pollen percentages are low and deciduous tree and pine percentages are high. In the next overlying zone black spruce attains a maximum; in the underlying zone white or red spruce, or both, was dominant. (Equals sample W-361, dated at 10,800 ± 250 yr, *8*). Submitted by Margaret Bryan Davis, Harvard University.	M-413	10,700 ± 800
IV. *Southeastern United States*		
Chattahoochee River, Fla. Submitted by R. P. Bullen.		
Charcoal from Fort Walton zone at site J-5 in a natural levee of the river. Archeological considerations suggest that the site is middle Fort Walton period in time. The dates seem very satisfactory.	M-392	550 ± 200
Charcoal from fiber-tempered pottery zone at site J-5 in a natural levee of the river. The zone is 5½ ft below that from which sample M- 392 (above) was taken. Since the sherds included 3 St. Johns Incised, 15 St. Johns Plain, and 186 fiber-tempered, it is believed that the date represents the end of the Orange period in Florida. The date, while earlier than anticipated, is reasonable.	M-394	3150 ± 250
Charcoal from site Ja-63 located beside an old channel (?) in bottomlands of river; 862 out of 907 decorated sherds are Kolomoki Complicated Stamped. Balance include Blakely Complicated Stamped and a few Weeden Island types. The date, while early, is not as early as those of samples M-49 and M-50 from the Kolomoki site itself (*1*, p. 667). These dates suggest that the Kolomoki "culture" of southwestern Georgia started relatively early.	M-396	1600 ± 250
St. Johns River, Fla. Animal bones from layer V at the Bluffton shell midden site. The sample was taken from well below the base of a plain, fiber-tempered zone and hence must be late, preceramic, Archaic in date. Since the date is much later than that indicated for plain fiber-tempered in coastal Georgia and also later than that indicated for terminal fiber-tempered in northwest Florida (see sample M-394 above), it seems to be incorrect. Testing was difficult because of the very small amount of carbon remaining in these bones. Believe this date should be disregarded. Submitted by R. P. Bullen.	M-264	2700 ± 500
Clarksville site (44Mc14), Mecklenburg County, Va. John H. Kerr (formerly the Buggs Island) Reservoir. Charcoal sample from a late Middle Woodland hearth area bordering on the transitional Late Woodland horizon. Excavated by Carl F. Miller and submitted by Frank H. H. Roberts, Jr., director, River Basin Surveys, Bureau of American Ethnology.	M-397	850 ± 250
Poverty Point Site, La. Minute fragments of charcoal collected by flotation method from the large ash bed that lay beneath the conical mound "B." Sample dates Poverty Point cultural phase and probably H. N. Fisk's stage C_1 channel positions of the Mississippi River system. Other portions of this sample were submitted to other laboratories: Lamont 272, 2700 ± 100 yr; Humble 66, 3150 ± 120 yr; Schatzman A, 2685 ± 210 yr; Schatzman B, 2339 ± 200 (*10*). Submitted by James A. Ford. The sample contained root fragments.	M-403	2850 ± 250
Calvert County, Md. Charcoal from site 18 An 18. Excavation sample No. 6 from 36 to 42 in. below base line. Submitted by T. L. Ford, Archaeological Society of Maryland.	M-418	1630 ± 400 2030 ± 250
Russell Cave, Jackson County, Ala. (site 1 Ja 181). Collected by Carl F. Miller, Smithsonian Institution.		
Charcoal from a stratum 8.0 to 8.5 ft below the present surface of the cave floor. At this depth the charcoal appears in small pockets and is associated with lithic tools, flint chips, and animal bones. Should equate with Middle Archaic.	M-589	8240 ± 400
Charcoal from a stratum of unctuous clay which lay at a depth of 12 to 13 ft beneath the present surface of the cave floor. The Lamont Laboratory has dated a similar specimen at 8160 ± 300 yr (*11*). This marks the beginning of the Archaic and the end of the Paleo Indian; in other words, the transitional blending of the two cultures.	M-590	8560 ± 400
Charcoal from a hearth area 5.5 ft from present surface. At this depth we are below the pottery-bearing levels, which we term the end of the Archaic. Bones from various animals, bone tools, and stone artifacts occur quite plentifully in this zone of occupation.	M-591	6300 ± 350
V. *Western and southwestern United States*		
Grand Canyon, Ariz. Wood of a split-twig figurine from a dry cave in the Redwall formation on south face of Grand Canyon. The figurine was in a buried cache with others approximately 10 in. below the surface of the cave floor. On the basis of distributional evidence it had been estimated that the figurines were older than A.D. 600. Collected by Douglas W. Schwartz, University of Kentucky, in August 1955.	M-563	3530 ± 300
Sandoval County, N.M. Charcoal from a series of hearths whose lithic artifacts fit into a Cochise-like culture. The area of concentrated hearths was 1000 yd east		

Description	Sample No.	Age (yr)
and west. Submitted by F. C. Hibben, University of New Mexico. Information from F. C. Hibben and George A. Agogino, Syracuse University. Earlier reports on three similar hearths are M-248, M-250 and M-251 (*1*, p. 670).		
Charcoal from a stone-lined hearth 16 ft beneath the present surface. A basalt metate was found in the hearth. This locality is 150 yards south of the site of sample M-248 and in the same arroyo.	M-249	3330 ± 300
Charcoal taken from a peripheral surface hearth located ½ mi to the north and west of the concentrated site area. The hearth was 4 yd across but produced no cultural material.	M-252	2180 ± 250
Charcoal from a surface location in the concentrated site area. This hearth was more than 12 yd in diameter. It was on an eroded hillside within 125 yd of the site of sample M-250 and on the same slope. A large number of tear-drop blades was found at this location.	M-253	2600 ± 300
Charcoal from a deeply buried lens in same arroyo as M-248 and M-249. This hearth is 19 ft below the present surface. No artifacts were found in it.	M-254	2900 ± 250
Stewart Rock Shelter, Clark County, Nev. Rectangle 2-B, 3-C. Mixed wood species. Charcoal taken from fire hearths, as indicated by concentrated charcoal deposits associated with cultural material. Submitted by Dick Shutler, Jr., University of Arizona.		
Feature No. 1. Depth 54 in. Hearth 4 in. thick, 15 in. in diameter. Thermally fractured rock present in hearth.	M-377	3870 ± 250
Feature No. 2. Depth 78 in. Hearth 5 in. thick, 12 in. in diameter, resting on the original ground surface of the shelter.	M-376	4050 ± 300
Santa Fe County, N.M. Charcoal from a firehearth exposed at the base of a 20-ft alluvial terrace of Rio Tesuque. Associated human artifacts indicated occupation by preceramic Basket Makers estimated to date between 1500 and 2500 yr ago. The top few inches of terrace contain pottery dating around A.D. 1200. These dates make possible the computation of the accumulation rate of sediments comprising the 20-ft terrace in this locality. Submitted by Fred Wendorf, Museum of New Mexico, and J. P. Miller, Harvard University.	M-511	2230 ± 250
Snaketown site, Gila River Indian Reservation, Pinal County, Ariz. Cordage and textile found in a pottery vessel, carbonized during the burning of a house. Field number: 6G: House #8, in Vessel #44 (*12*). Collected by staff of Gila Pueblo about 1935. Considered to be of Sacaton phase of Sedentary period.	M-324	700 ± 250
Cave near Kingman, Mohave County, Ariz. Food cache of "mescal" from pottery vessel with lid hermetically sealed with lac. Collected by an amateur archeologist in 1938 and deposited in the Museum of Northern Arizona, catalogue number 1019/L. (*13*). The vessel is apparently of the ware known as Pyramid Gray and was found in the geographical region of the Cerbat Branch of the Patayan Root.	M-381	650 ± 200
San Carlos Indian Reservation, Point of Pines area, Ariz. W:10:112 (Arizona State Museum Survey). Submitted by Emil W. Haury.		
Scattered fragmented charcoal from bed D-1 at a depth of approximately 3 m under valley sediments; associated with Chiricahua stage cultural material.	M-461	2600 ± 250
Fragmented solid charcoal from matrix of pit 3, bed C-2, distributed between cremations. May be derived from crematory fires brought to this location with the ashes. Small sample; run may not be very accurate.	M-462	1140 ± 300
Charcoal highly fragmented, distributed through matrix of bed D-1; two field samples from a 3-m area combined (Univ. of Arizona Carbon-14 Age Determination Laboratory Nos. 21 and 22).	M-540	2400 ± 200
Fragmented charcoal of pine and oak from hearth in bed D-1 at a depth of 2.75 m under valley sediments; associated with Chiricahua stage cultural material. (Univ. of Arizona Carbon-14 Age Determination Laboratory No. 19).	M-541	2530 ± 250
Ten Sleep, Wyo. Charcoal sample from single occupation level of cave about 10 mi south of Ten Sleep. Associated with large complex of Late Middle period perishable and nonperishable artifacts, including atlatls, foreshafted atlatl darts, fire drills, basketry, hafted knives, scrapers, projectile points, and other items. This date probably reflects a period near the end of the Late Middle occupation and fits the previously projected chronology for this manifestation. Submitted by George Frison and William Mulloy, University of Wyoming.	M-433	1725 ± 200
James Allen site near Laramie, Wyo. Burned *Bison occidentalis* bone from abbatoir site on the north side of Boulder Ridge, 16 mi south of Laramie. Associated with a number of *Bison occidentalis* individuals, projectile points which have been variously called Browns Valley points, Oblique Yumas, and so forth, and a complex of stone tools including plano-convex scrapers, ovoid and piriform knives, choppers, retouched flakes, and other items. Represents one of the hitherto unfixed complexes of the Early period. Submitted by William Mulloy, University of Wyoming.	M-304	7900 ± 400
Falcon Reservoir, Starr County, Tex. Site 41-78B9-4. A composite sample of hundreds of minute pieces of charcoal was taken from the hard, brown, adobe matrix of the lowest occupation zone (zone I). This zone was 4 to 7 in. thick at a depth of 8 to 9 ft below the surface. It was primarily a workshop area. Associated deer bone fragments were partly mineralized. Projectile points are large, thinned base, elongate, triangular forms and all of rather uniform size and shape. The site is situated along a major arroyo some 300 yd from its confluence with the Rio Grande River. A date for this carbon should suggest a rate of deposition for this particular Rio Grande terrace as well as a time-span for the cultural materials. Artifacts are similar in zone I to those found in nearby surface sites. A cultural complex here appears to extend from the time of the zone I occupation to relatively recent times with but little change. Collected in March	M-129	4650 ± 300

Description	Sample No.	Age (yr)
1951 by Donald D. Hartle and submitted by Robert L. Stephenson, River Basin Surveys, Smithsonian Institution.		
Temecula, Calif. Carbon from a fireplace on the floor of Ramada 1, at a depth of 30 in. in a Luiseno site of the Shoshonean Culture. Located on a bench above the Temecula River, 1 mi south of Temecula. Submitted by B. E. McCown, San Diego, Calif.	M-245	< 250
Winnemucca caves, Pershing County, Nev. The two dates below are but a small part of the radiocarbon dating being done in the Winnemucca caves of Lake Lahontan. Other dates have been made by Lamont, and while the Michigan measurements do not duplicate any specimens measured by Lamont, they do supplement them. Submitted by Phil C. Orr, Western Speleological Institute, Santa Barbara, Calif.		
Crypt Cave. Twisted bird skin robe from Indian burial, Nevada State Museum No. P3a/127. This is a well preserved mummy, with perishable buckskin and basketry, from a cave on the upper dendritic level of ancient Lake Lahontan. A coiled basket superimposed over the grave was dated by Lamont at 2400 yr (L-289DD) (*14*), under circumstances which suggest a reinterment of the older basket. No conflict of radiocarbon dates is seen. Submitted by Phil C. Orr.	M-436	1510 ± 200
Chimney Cave. Cedar bark matting from burial, Nevada State Museum No. P3b/198. An excellent female mummy flexed, face on side, wrapped in cedar bark robe and animal skins, untanned hair removed. Pubic apron of untanned fringed skin with cordage about the waist. Burial of this body disturbed an older one (No. 3) and was later covered with cactus brought in by rats, representing a second cactus level for the Winnemucca caves. The earliest cactus level is associated with extinct horse bones from nearby Crypt Cave.	M-437	2040 ± 250
Lower Columbia River, Ore. A large midden accumulation near The Dalles. Test trenching has revealed stratigraphy which analysis has shown to have cultural significance. The lower stratum (A) has been disturbed by, presumably, fluvial or near-fluvial action and by pit house building. It contains the remnants of split cedar structures and a culture which appears to have coastal affinities. Stratum B, overlying A, contains a heavy accumulation of artifacts which are Plateau in their cultural character. Projectile points and stone carvings are typical of the latest periods. Collected by Warren Caldwell; submitted by Douglas Osborne, University of Washington.		
Split cedar plank, from a structure intrusive into the oldest levels of the mound (in test trench 3—#44), stratum A.	M-410	1090 ± 200
Charcoal sample from test trench 2 (#32). From same level as the preceding, but not from one of the older, undisturbed pockets of original midden (which have not yielded a datable sample).	M-409	1070 ± 200
Wood post, charred, from test trench 3 (#40), stratum B.	M-407	560 ± 200
Charcoal from test trench 2 (#15), same stratum as preceding.	M-406	900 ± 200

Description	Sample No.	Age (yr)
VI. *Mexico and Guatemala*		
Portales Cave, southwest Tamaulipas, Mexico. Collected by D. Kelley and submitted by R. S. MacNeish, National Museum of Canada.		
Vegetable materials from level 3, square S10 of cave Tm c 248. These were associated with Ocampo culture artifacts as well as gourds, squash (pepo), common beans, and some sort of small lima-like bean. They were definitely above level 7 from an adjacent square dated as 8200 ± 450 yr (M-498) and under materials in a nearby pit from above level 3 dated as 3945 ± 334 yr by the University of Chicago. Ocampo remains from excavations in the cave (Tm c 247) nearby have been dated as 5230 ± 350 yr (M-504) and 4580 ± 350 yr (M-503). Comparable material from the Sierra de Tamaulipas (M-487) is 4445 yr old, and Falcon Dam of Texas (M-129) is 4665 yr old. This is only slightly older than expected.	M-497	5650 ± 350
Vegetable materials from level 7, square S10E10 of Tm c 248. These were associated with Infiernillo artifacts as well as gourds, squash, and some sort of bean (maybe wild). These were under M-497. These are of the same culture as that of M-500 from a nearby cave dated as 8540 ± 450. This is older than expected but in light of its cross-dating and agreement with stratigraphy, as well as the fact that there was nothing underneath it that could contaminate it, one has to accept it as valid.	M-498	8200 ± 450
Diablo Cave, Sierra de Tamaulipas, Mexico. This dates carbon from floor X, layer 5 of square S10E5 of Tm c 81. Associated with floor X were stone tools of the Lerma phase. In cave Tm c 174 Lerma remains were below a sterile layer below vegetable materials dated (M-487) as 4445 yr ago and typologically it seems older than Infiernillo phase in southwest Tamaulipas dated as 8200 and 8540 yr. The date is acceptable. Collected and submitted by R. S. MacNeish.	M-499	9270 ± 500
Ojo de Aqua Cave, southwest Tamaulipas, Mexico. This carbon from the lowest hearth area of square S30E5 in level 11 at a depth of 5.3 ft below the surface of cave Tm c 274. The cultural materials are Infiernillo phase (like M-498) though they are directly under Flacco remains dated as 3945 yr old by the University of Chicago. It is also under Palmillas remains (M-506 and M-568) which underlie San Lorenzo vegetable stuffs (M-501). The date is older than expected but, in light of other dates and stratigraphy, is acceptable. Collected by D. Kelley and submitted by R. S. MacNeish.	M-500	8540 ± 450
Vegetable material from level 1, 0.9 ft below the surface in square N15W5 of cave Tm c 274 and from a layer overlying carbon of M-500. The artifacts are of the latest prehistoric phase of the area, called the San Lorenzo phase, and more or less related to the mixed specimen from the Sierra de Tamaulipas (C-207) dated as 651 years old (*5*, p. 129). It was as expected. Collected and submitted by R. S. MacNeish.	M-501	520 ± 200

Description	Sample No.	Age (yr)
Romero's Cave, southwest Tamaulipas, Mexico. Vegetable materials from level 11, occupation 2, square S30E5 of Tm c 247. The associated artifacts were of the Ocampo phase (see M-497 and M-503), and the sample was under M-503, M-504, M-505, M-506, M-567, and M-568 from the same profile (E5). The date is acceptable in terms of the stratigraphy and other dates for this culture. Collected by P. Grant and R. S. MacNeish and submitted by R. S. MacNeish.	M-502	5230 ± 350
Vegetable materials from level 8, occupation 5, of square S25E5 of Tm c 247. Associated with Ocampo tools. In terms of M-497 and M-504, the date is acceptable. Collected by Peter Grant and R. S. MacNeish and submitted by R. S. MacNeish.	M-503	4580 ± 350
Charcoal and vegetable material (the two samples were combined) from level 6, occupation 7 of square S20E5 of Tm c 247, associated with Guerra phase materials, including Bat Cave type corn. In terms of the stratigraphy, the date is too early. Collected and submitted by R. S. MacNeish. Contained much sand and dirt.	M-504, M-567	4730 ± 300
Vegetable material from top of level 4, occupation 9 or 10 of square S20E5 of Tm c 247. Level 4 was a thick layer in the back of the cave, often divided by a lens of ash into level 4A and 4B. Level 4A contained sherds of the Mesa de Guaje phase and many agricultural products, while Guerra in 4B was preceramic. When there was no middle ash lens, sherds appeared in its upper portion but not in its lower; however, the two parts were not readily distinguishable. These specimens came from the upper portion and should have been deposited by the Mesa de Guaje phase. However, the date is earlier than expected, and I expect that it includes quite a bit of Guerra refuse. This in reality probably represents a maximum date for Mesa de Guaje and a minimum for Guerra. In terms of its stratigraphic position between M-506 and M-503 it seems to fit very nicely. Collected and submitted by R. S. MacNeish.	M-505	3650 ± 250 3440 ± 250
Vegetable materials from level 3, occupation 11, of square S20E5 of Tm c 247. It is associated with Palmillas remains, the cultural and agricultural apogee of the area. It seems to be related to other late Classic remains in Mexico. In terms of the stratigraphy, the date is acceptable but comparatively it seems slightly earlier than expected. Collected and submitted by R. S. MacNeish.	M-568	1720 ± 200
La Venta, Tabasco, Mexico. Samples collected during the 1955 National Geographic Society-Smithsonian Institution-University of California excavations from the rectangular "Ceremonial Court" (Complex A), which lies just north of the Great Pyramid (*15*). Complex A was found to have four major construction (*not* cultural) phases, which were assigned the numerals I (earliest) to IV (latest). The dates obtained from these samples are interpreted as indicating that complex A was constructed and used during the period from approximately 2755 to 2355 yr ago (about 800 B.C. to 400 B.C.), justifying suspicions of the early occurrence of the La Venta–Middle Tres Zapotes period of Olmec culture. The 1955 excavations at La Venta bear out fully Drucker's equation, as based upon pottery analysis of the La Venta site, with Middle Tres Zapotes. Submitted by Robert F. Heizer, University of California.		
Charcoal from phase I floors at northeast corner of southwest platform.	M-535	3110 ± 300
Charcoal from phase I stage of northeast platform.	M-529	2860 ± 300
Charcoal from bottom of phase II pit 68 in. below surface of northwest platform.	M-530	2760 ± 300
Charcoal from artificial fill underlying and contemporaneous with phase I floors in northwest platform.	M-534	2670 ± 300
Charcoal from phase I platform in mound A-2.	M-532	2650 ± 300
Charcoal from leveled base sands underlying and contemporaneous with phase I platform in mound A-2.	M-531	2560 ± 300
Charcoal from lower margin of post-complex A occupation windblown sands lying on phase IV surface west of northeast entryway.	M-528	2400 ± 250
Charcoal from burned area on phase IV surface west of limestone slab paving near northeast entryway. Interpreted as evidence of early post-complex A activity by people other than the builders of the La Venta site.	M-533	2130 ± 300
Charcoal from the bottom of a trench cut into north apron of the Great Pyramid. Should give the date of the early (perhaps the initial) construction of the pyramid.	M-536	2530 ± 300
Chimaltenango Department, Guatemala. Natural charcoal from within and under volcanic ash deposit at the headwaters of Rio Madre Vieja, about 12 km west of Patzun. Submitted by L. C. Stuart, University of Michigan.	M-292	24,000 ± 3000
La Quemada, near Zacatecas, Mexico. Submitted by James B. Griffin, University of Michigan.		
Construction timber, probably a roof beam from unexcavated fill on north side of a room in the east side of the Acropolis.	M-430	890 ± 200
Charcoal selected from a concentration of charred wood against a smoke-stained wall in a room of the Acropolis excavation.	M-431	780 ± 200
Construction timber from upper western section of occupied area.	M-432	1210 ± 200
Frightful Cave (CM68) Coahuila, Mexico. The site is 15 mi southeast of Cuatro Cienegas. Collected and submitted by W. W. Taylor, Jr., Mexico City.		
Miscellaneous wood fragments from middle level.	M-192a	9540 ± 550
Wood fragment W283 from middle level.	M-192b	9300 ± 400
Miscellaneous wood remnants from top level.	M-193	3200 ± 250
VII. *Pacific and Far East.*		
Able Site, Kapyong, Korea. Charcoal sample from a charred log representing the roof structure of a protohistoric pit-dwelling in central Korea. Other material found includes stone tools and ornaments, as well as pottery made both by coiling and on the potters' wheel. The latter pottery appears quite like ware of the Han	M-303	1700 ± 250

Description	Sample No.	Age (yr)
Dynasty of China and probably belongs to the Lolang era in Korea. This sample dates a house of what must have been frontier-living peasants who still used Neolithic tools as well as imported bowls. Scraps of rusted iron tell that this material was known and used as well. The material found is very much like that from house pits of similar age in northern Japan. Submitted by Howard A. MacCord, U.S. Army Engineers.		
Hawaiian Islands. Charcoal samples collected in 1955 by Bernice P. Bishop Museum parties under the direction of K. P. Emory, and submitted by him.		
Charcoal from bluff shelter, Haeleele, Kauai Island, site K 1, square E 32, at the very bottom of a cultural deposit extending from 3 to 42 in. below the surface.	M-477	520 ± 200
Charcoal from large lava-tube shelter named Makalai, South Point, Hawaii Island, site H 2, square S 9, at 48 in. depth in a cultural deposit extending from 3 to 53 in. below the surface.	M-478	300 ± 200
Charcoal from a ground oven apparently under an ancient house floor buried by a sand dune at South Point, Hawaii Island, site H 1, square J 5. The house floor must date not later than A.D. 1800, for no post-European period artifacts were found in it.	M-479	200 ± 200
Charcoal from a bluff shelter, Nihoa Island, 150 mi northwest of Kauai Island, site 60, at 14 to 18 in. below the floor, being sterile above and below this fireplace. Collected by H. Ivan Rainwater.	M-480	520 ± 200
Charcoal from a fireplace under the ancient house floor at South Point, Hawaii Island, site H 1, square L 11, at 14 to 17 in. below the top of the cultural deposit. We believe the probable age of this fireplace to be at least 400 yr, and suspect, as with M-479, contamination through sea-water or rootlets.	M-538	< 200
Yap Island. University of California expedition of 1956, E. W. Gifford and D. S. Gifford, University of California, Berkeley.		
Charcoal from grave of Rugog, Noah of Yapese mythology, Teb village, Tomil municipality. University of California Museum of Anthropology (UCMA) No. 11-32906.	M-626	200 ± 200
Charcoal from depth of 24 to 30 in. at site of Penin, Kanif village, Dalipebinau municipality. Cultural refuse extended to a depth of 30 in. UCMA No. 11-32781.	M-629	200 ± 200
Charcoal from depth of 30 to 42 in. at house site of Boldanig, Malaj village, Kanifay municipality. Cultural refuse extended to a depth of 90 in. UCMA Nos. 11-32794, 32806, 32818, 32830, 32842.	M-631	320 ± 200
Charcoal from depth of 18 to 24 in. at site of Pemrang, Giror village, Galiman municipality. Cultural refuse extended to a depth of 90 in. UCMA No. 11-32894.	M-632	250 + 400, −250
Charcoal from depth of 24 to 30 in. at same site as M-632. UCMA Nos. 11-32862, 32871, 32878, 32886, 32895.	M-633	100 + 200, −100
Charcoal from depth of 48 to 72 in. at same site as M-632. UCMA Nos. 11-32863 to 32866, 32874, 32881, 32887.	M-634	1780 ± 250

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News of Science

NSF Appropriation

The U.S. House of Representatives has approved a budget of $115 million for the National Science Foundation, as compared with the Administration's request for $140 million. In effecting a net reduction of $25 million below the President's budget amount of $140 million, the House followed the report of its Committee on Appropriations in recommending $107 million for continuing programs (all funds requested, except $5.3 million for research facilities) and $8 million for "the most promising new programs."

The full request for $40 million for grants for basic research was approved. The House specifically disallowed funds for the Southern Hemisphere astrograph, solar research telescope, university research, reactor and computing facilities, and the optical astronomy observatory.

The full request for $54,220,000 in support of scientific manpower under the continuing programs was approved. The request for $27 million for new programs in this area was disapproved, though the programs were not specifically disallowed.

It is expected that there will be an effort before the Subcommittee on Independent Offices and General Government Matters of the Senate Committee on Appropriations to restore the $25 million cut by the House. Senator Warren G. Magnuson of the state of Washington is chairman.

The Erect Posture and the Skull

Much has been written about the position of the foramen magnum as an index for determining the posture of such fossil primates as the australopithecines of South Africa and of Neanderthal man. A recent intensive and extensive comparative study of the skulls of living primates by J. Biegert [*Morphol. Jahrb.* **98**, 77 (1957)] is pertinent to this problem. Biegert concludes that changes in the skull during human phylogeny have been bound up with two evolutionary tendencies: (i) an increasing development of the brain and (ii) an increasing reduction of the masticatory apparatus. The cranium became higher, the frontal bones elevated, and the supraorbital torus reduced as the orbits and jaws increasingly came to lie beneath the presellar brain-space; and the facial profile became more orthognathous as the sagittal bending of the cranium increased. Concurrently, the foramen magnum was displaced ventrally.

Biegert therefore concludes, in opposition to Weidenreich (1924) and Clark (1955), that acquisition of the erect posture did not markedly influence the structure of the skull, but that the ventrally displaced foramen magnum is the result of changes initiated by extreme cerebral development. Thus he believes that an erect posture cannot be assigned to a fossil hominoid, such as an australopithecine, on the basis of skull structure or position of the foramen magnum. This can only be determined from the postcranial skeleton, above all, from the pelvis.—W. L. S. JR.

Barenblatt Case and Congressional Investigations

The U.S. Supreme Court agreed on 15 April to review the case of Lloyd Barenblatt, former Vassar College psychology instructor who was convicted of contempt of Congress for refusing to answer questions by the House Un-American Activities Committee 4 years ago about alleged Communist connections. Barenblatt's case is a sequel to the Watkins case of last year.

The Court reversed the contempt conviction of union organizer John T. Watkins on grounds that the House Committee should have, but did not, tell Watkins how questions put to him were pertinent to its legislative function. At the time the Court majority also criticized the vagueness of the House resolution creating the Un-American Activities Committee and spoke of possible infringement of constitutional rights. In another case decided the same day some of the justices spoke of the need for education to be free from pressures of investigation of speech and beliefs.

The Supreme Court sent Barenblatt's conviction back to the Court of Appeals for the District of Columbia to decide how it conformed with the Watkins decision. By a 5–4 vote the Court of Appeals in January reaffirmed the conviction. The majority held that the pertinence of questions was made clear to Barenblatt. The four dissenting justices felt that the Supreme Court had outlawed investigations in the field of education. Two of them also felt that the Court had struck down the resolution creating the House Un-American Activities Committee, thus stripping it of power to investigate anything.

Barenblatt's Supreme Court appeal asks whether or not the Court did invalidate the establishment of the Un-American Activities Committee, whether or not Congress had authorized the committee to investigate education, whether or not such an investigation is constitutional, and whether or not he was told the pertinence of questions.

Other cases raising similar questions and involving the Senate Internal Security Subcommittee and state antisubversive investigations are before or on their way to the Supreme Court.

Television and Film Instruction

The potential benefits and dangers of secondary-school instruction by films and television are examined in a report published recently by the Advisory Board on Education and the Division of Mathematics of the National Academy of Sciences–National Research Council. Although the report, prepared by a specially appointed Film Evaluation Board, addresses itself only to films and kinescopes prepared for the teaching of mathematics, many of its observations and recommendations might be applied to other sections of the secondary-school curriculum. The report, entitled *Films and Television in Mathematics Education,* was based on a joint viewing by the board of most of the mathematics films and kinescopes now available for teaching purposes. Several days of continuous sessions were required for the assignment. The board reported that:

"There is little doubt that the more specific, more tangible needs of group instruction can be met acceptably by recorded sequences of sufficiently high quality. It is not essential, either, to sacrifice entirely the less specific, less tangible aims. A carefully prepared recorded sequence, especially done by an expert, may in fact convey healthier scientific attitudes and deeper insights than a routine classroom lecture by a teacher who is uninformed, unresponsive, or otherwise inept."

On the other hand, the board also saw the possibility of "wide dissemination of erroneous ideas and unfortunate pedagogical stereotypes."

"Mass media entail a heavy responsibility. A single misunderstanding communicated in a presentation to a large group of students can handicap the efforts of all the teachers who must deal with the students personally."

Acknowledging the concern of many educators lest the use of instructional film series seriously undermine the traditional and vital personal interchange between teacher and student, the board pointed out that, on the contrary, proper scheduling of recorded material can actually release a faculty for more individual contact with students than is now permitted by many teaching schedules. Further, it was the unanimous opinion of the board that in "an extended system of presentations offered in connection with a more or less conventional academic course . . . not more than half the time allotted to formal group instruction should be used for presentations [and] that such presentations should be devoted primarily to the exposition of basic ideas and principles. . . ."

Chairman of the Film Evaluation Board was F. A. Ficken, University of Tennessee. Other members were A. M. Gleason, Harvard University; T. H. Hildebrandt, University of Michigan; G. Hochschild, Institute for Advanced Study, Princeton, and the University of Illinois; J. D. Mancill, University of Alabama; and B. E. Meserve, State Teachers College, Upper Montclair, N.J.

In reporting to the Academy–Research Council, the board recommended the establishment of a standing committee on mathematical presentations to offer guidance to schools and film producers and to promote the participation of professional mathematicians in the program. The board also advised formation of similar committees in other fields of the natural sciences. Formation of such committees has already been undertaken.

Atherosclerosis Research

A baboon airlift from Africa to Texas began last month when the Southwest Foundation for Research and Education in San Antonio imported 20 baboons from Nairobi, Kenya, for research on atherosclerosis. The baboon is the only mammal other than man that is subject to the fatty deposits that characterize atherosclerosis. The foundation already has 30 domestically bred baboons. It plans to increase the colony to 1000 this year. The rapid expansion of the baboon program has been made possible by a recent gift to the foundation of $50,000 by Douglas Marshall, a Houston oil man who is chairman of the Texas Heart Research Foundation.

Foundation scientists have joined with investigators from three other institutions to form a group that is known as Cooperative Research on Atherosclerosis. The group includes research workers from Louisiana State School of Medicine, New Orleans; the Oklahoma Medical Research Foundation, Oklahoma City; and the Enzyme Institute of the University of Wisconsin.

Soviet Antarctic Station

Gordon D. Cartwright, meteorologist for the U.S. Weather Bureau, has returned to Washington, D.C., after spending 14 months on an International Geophysical Year assignment at the Soviet Antarctic IGY station at Mirny. Cartwright joined the Soviet expedition at Capetown on the 26 December 1956 and left it at Adelaide, Australia, on 4 March 1958. In describing his winter at the Mirny Station, where he was the only American among more than 100 Russians, Cartwright said:

"This was the most stimulating experience of my life. Antarctica by itself makes a vivid and lasting impression on anyone who touches it, and in addition I had an unusual opportunity to observe a group of Russians at work and off duty. I found them warm, hospitable, and with broad scientific and cultural backgrounds. They had a keen sense of humor and their differences of outlook were, of course, sometimes delightful and sometimes difficult to understand."

Cartwright joined in the regular weather analysis work at Mirny, where he was responsible for the preparation of daily upper-air charts covering most of the Southern Hemisphere. The IGY network of observing stations in the Antarctic has made possible for the first time in history the drawing of reliable weather charts of the South Polar regions. Hundreds of cloud photographs and several thousand feet of time-lapse color pictures of special cloud developments in the polar region were taken by Cartwright.

The Soviet expedition is operating in one of the most difficult and least known areas of the Antarctic. The interior stations are located near the central dome of the East Antarctic ice plateau at elevations near 12,000 feet. In establishing these stations the Soviet group was faced with two major problems: the "height-cold barrier," a combination of intense cold and great height that places exceptional strain on both men and equipment; and "soft snow," which can bog down the most powerful tractors and can make ski landings of aircraft highly dangerous except on specially prepared runways.

Cartwright reports that the height-cold barrier and soft snow prevented establishment last year of two stations the U.S.S.R. had planned in the deep interior. However, by a massive effort during the recent Antarctic summer, and by using new equipment designed on the basis of last year's experience, both stations are now in full operation.

The U.S.S.R. is now operating six scientific stations in East Antarctica. Mirny, which is the main station, is on the Antarctic Circle at 93° East Longitude. A second Soviet station is located on the edge of the ice sheet at Bunger Oasis, and a third station, called Vostok, is in the region of the south geomagnetic pole. The newest station, Sovietskaya, is near the so-called "pole of relative inaccessibility." Two smaller U.S.S.R. observation stations lie on the tractor route to Vostok and Sovietskaya. The leader of the Soviet expedition, Alexei Fyedorovich Troshnikov, is well known for his work as a hydrologist in Arctic regions. The chief meteorologist, Oscar Grigoriovich Krichak, is a member of the Central Forecasting Bureau in Moscow.

Cartwright was especially impressed by the well-equipped ships, the *Ob* and *Lena,* which served not only as major transport ships for the U.S.S.R. expedition but also aided some of the most comprehensive oceanographic surveys and observation work ever made in Antarctic waters.

The scientific exchange in which Cartwright took part was so successful that U.S.A. and U.S.S.R. scientists have agreed that similar arrangements should be continued for another year. Morton J. Rubin, also of the Weather Bureau, is already at Mirny, where he will spend the next year doing meteorological work.

Atomic Clock Discrepancy

A discrepancy exists in radio comparison of British and American atomic clocks. These clocks, whose operation is based on the unvarying vibrations of the cesium atom, are accepted as the most accurate measurement standard availble. The atomic clock, or Atomichron, has a possible accuracy down to one part in 10^9. Last summer the frequency of radio signals controlled by the cesium standard at the National Physical Laboratory, Teddington, England, varied by nine parts in 10^9 from the frequency of similar equipment in this country.

In an effort to solve the difference, two Atomichrons from the Army Signal Laboratories at Fort Monmouth, N.J., have been sent to Teddington for comparison. A third clock has been sent to Cruft Laboratory at Harvard University for radio checks with Teddington.

The Atomichron, which in terms of time has an accuracy of one second per 300 years, is of great importance to the

Army Ballistic Missiles Agency at Huntsville, Ala. One clock has been installed in the agency's Guidance and Control Laboratory, where the systems that steer rockets in flight are developed, and two others are used by the Missiles Firing Laboratory, the unit that launches the Army rockets from the Test Center at Cape Canaveral.

Vanguard to Last 200 Years

John P. Hagen, director of the Vanguard Project, estimated in a recent speech before the American Society of Newspaper Editors that the life expectancy of the Vanguard satellite was "at least 200 years." Shortly after the launching last month Hagen predicted that the 6-inch sphere would last for at least 10 years. Vanguard is on a steady course—405.1 miles from the earth at the nearest point and 2463 miles away at the most distant—and is circling the earth in 2 hours, 14 minutes, and 4 seconds. The change in orbit has been so small "that it is most difficult to measure," according to Hagen.

It is estimated that the Army's Explorer I, the first United States satellite, will last from 3 to 5 years. Explorer II did not orbit, and Explorer III was given a life expectancy of "at least two months" when it was launched on 26 March.

News Briefs

Children are still immune to poliomyelitis 3 years after their original inoculations with Salk vaccine. This finding was announced on 15 April in a report to the American Association of Immunologists by Gordon C. Brown, professor of epidemiology at the University of Michigan. Brown's report was based on a study of 139 children. He said that infants who had received smaller-than-average doses of the vaccine 3 years ago are still protected, too. The study also showed that the booster shot is the most important inoculation in the entire poliomyelitis series.

* * *

Scientists who are working in the fields of aviation and space medicine but who are not physicians may now become full members of the Aero Medical Association in accordance with amendments to the society's constitution and bylaws adopted at the 29th annual meeting at the Statler Hotel, Washington, D.C., on 25 March. In the past, aeromedical scientists who did not possess the degree of doctor of medicine were eligible only to become associate members.

* * *

The National Academy of Sciences has announced that the *IGY Bulletin*, official monthly publication of the U.S. National Committee for the International Geophysical Year, is now available by subscription. The subscription rate is $4. This will include all back issues, dating from July 1957, together with all future issues. (The *Bulletin* will be published at least through December 1958 and possibly through June 1959.) Subscriptions should be sent to the Publications Office, National Academy of Sciences, 2101 Constitution Ave., Washington 25, D.C.

* * *

A new international vocabulary of lighting terms, the culmination of 20 years of study by a working party of the Commission Internationale de l'Eclairage, is now ready for distribution through the organization's United States National Committee. Printed in three languages—French, English, and German—the *International Lighting Vocabulary of the International Commission on Illumination* contains 530 terms, with definitions, as well as numerous symbols and formulas. The publication may be obtained for $2.50 from Mr. T. D. Wakefield, Treasurer, U.S.N.C. Vermilion, Ohio.

* * *

The American Phytopathological Society has for several years sponsored the publication of results from tests on new fungicides. The *Results of 1957 Fungicide Tests* for the first time has been printed privately and is issued as a single publication. Previously the *Results* have been assembled by combining reprints of serial articles published in *Agricultural Chemicals*. The *Results of 1957 Fungicide Tests* can be obtained for $1 per copy from Dr. A. B. Groves, Department of Plant Pathology and Physiology, Winchester Fruit Research Laboratory, Route 3, Winchester, Va.

* * *

Present knowledge of the geology and mineral resources of the continental shelves of North and South America is summarized in a report released recently by the U.S. Geological Survey. The report consists of a set of four papers, under the over-all title *An Introduction to the Geology and Mineral Resources of the Continental Shelves of the Americas*, by James Trumbull, John Lyman, J. F. Pepper, and E. M. Thomasson. Copies may be obtained for 75 cents each from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C.

* * *

Only 16 secondary schools in the United States—eight public and eight private—are now teaching the Russian language, according to a report by Helen B. Yakobson, head of George Washington University's Slavic languages department. However, all but seven states have at least one college or university offering Russian. By way of contrast, from 5 million to 6 million Soviet students are reported to be studying English, and Russian is taught in 70 French secondary schools.

Scientists in the News

ROBERT B. BRODE has been named associate director for research at the National Science Foundation, effective in July. He will be on leave of absence from the University of California (Berkeley), where he has been professor of physics since 1932. He succeeds PAUL E. KLOPSTEG, who will continue to serve the foundation as a consultant. Klopsteg is president-elect of the AAAS.

ARTHUR E. LILLEY, assistant professor of astronomy at the Yale University Observatory, has been awarded the Bart J. Bok Prize for his work in radio astronomy. The award was made on 22 April at a Harvard University conference on radio noise. Lilley's work deals with measurements of the doppler effect in the radio spectrum.

The Bok Prize is awarded every 2 years to a student who has recently been awarded the Ph.D. in the physical sciences at Harvard or Radcliffe. It is given for "work in the area of Milky Way research by observational methods." The prize fund was donated anonymously in 1956 in honor of Bart J. Bok, longtime professor of astronomy at Harvard, and now director of the Mount Stromlo Observatory of the Australian National University.

JOHN P. SCOTT, chairman of the division of behavior studies at the Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Me., since 1945, and senior staff scientist since 1957, joined the department of psychology in the division of biological sciences of the University of Chicago on 1 April, as visiting professor for the quarter ending 30 June.

The following awards were made during the 95th annual meeting of the National Academy of Sciences, which took place in Washington, D.C., on 28 April.

HORACE W. BABCOCK, astronomer, Mount Wilson and Palomar Observatories, Pasadena, Calif., received the Henry Draper Medal "for his original and outstanding work leading to the discovery of magnetic fields in stars and also the general magnetic field of the sun."

MARK G. INGHRAM, professor of physics at the Enrico Fermi Institute for Nuclear Studies, University of Chicago, received the J. Lawrence Smith Medal "for his work on the measurement of the ages of meteorites."

GUSTAV A. COOPER, head curator, department of geology, U.S. National Museum, Washington, D.C., re-

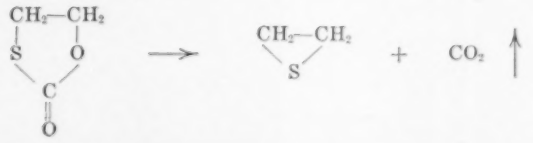
Kodak reports on:

a sm[illegible]y but stable invention . . . creation and propagation of slides and filmstrips . . . tami[illegible] temperature variations

Pres[illegible] ethylene sulfide

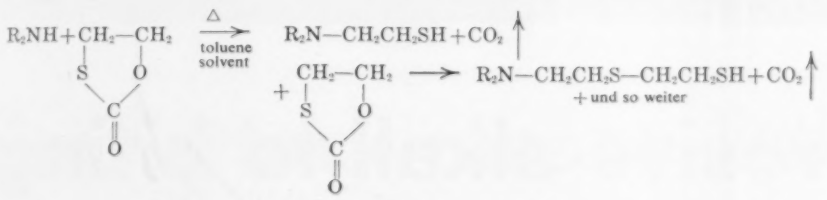
Talk about your polymer success stories. Polyethylenesulfide $(—CH_2CH_2S—)_n$ is one that never made good. Nor for want of trying. Been subject of much literature. Few suitable solvents. Monomer, ethylene sulfide ($CH_2—CH_2$ with S bridging) is clear, volatile liquid (BP 56). Doesn't stay that way long. Maybe a day if you play your cards right. Used to be prepared by reacting 2-chloroethylmercaptan with sodium bicarbonate. Ethylene sulfide has to be fractionated from the water. Most of it polymerizes on the way. Russians do it by reacting ethylene oxide with potassium thiocyanate. Long drawn out and messy. Tried it and got nowhere near the 25-50% yields they claim. May be our fault.

Anyway, spent long time looking for better idea. Wound up inventing "new composition of matter." Call it *Ethylene Monothiocarbonate*. Nice, smelly but stable liquid. Keeps long time below 200 C. Get it that hot and presto, ethylene sulfide. So:



96% yield.

Ethylene sulfide long considered *sine qua non* of mercaptoethylation. Now you can mercaptoethylate *sine*. Life thus made simpler, more satisfying, safer because at temperature required for attachment of $—CH_2CH_2SH$ group, ethylene sulfide is far above boiling point but *Ethylene Monothiocarbonate* is not. Reaction proceeds peaceably without pressure plumbing and all that jazz. Mercaptoethylation might be pushed onward and onward. Viz.,



Long polyethylenesulfide side chains have been hooked onto NH_2 or SH groups of proteins and onto the OH groups of cellulose. Also talk in literature of entwining natural polymer chains with polyethylenesulfide chains formed *in situ* but not bonded to them.

Glad to sell you Ethylene Monothiocarbonate. *From $3.10 for 10 grams on up in quantity and down in price. Ask for Eastman 7367. Can supply approximately 3600 other Eastman Organic Chemicals at same time. Convenient. Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company).*

The powers of semi-darkness

One result of all the efficiency pervading life today has been more time to sit around in semi-darkness listening to speakers draw attention to Worthwhile Matters with the help of slides or filmstrips. You will not deny that this is good.

What, then, can be done to encourage and facilitate the generation of lots more slides and filmstrips? Consider the sources.

There are organizations—some for profit, some non-profit—that produce them for schools. All they need for encouragement is sales. Subjects are of the kind that keep well. Life along the Nile. The circulation of the atmosphere. That sort of thing.

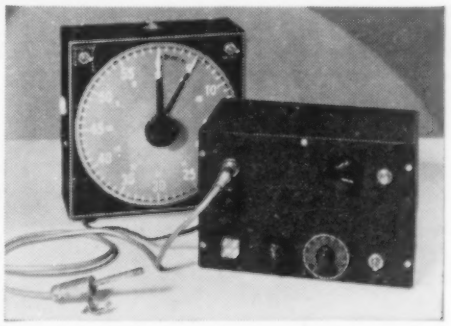
Then there are firms producing films and filmstrips to order for promoters of causes. The need for higher protective tariffs. The need for lower barriers to international trade. How to sell bicycles to people over 40. Here an advertising or public relations agency often acts as intermediary between sponsor and producer.

Outfits that use slides and filmstrips to communicate on a broad and varied scale often maintain their own production facilities for the purpose. This would include large companies, government bureaus, ag colleges.

Not to be neglected beyond these large operations, however, is the individual on his own who has an audience to face and to tell of his work and thoughts as vividly as he can. He, too, can make them—slides if he intends to put on the same performance only once or a few times, filmstrips if it is to be given many times in essentially unchanged form.

For his benefit we have published a Kodak Data Book, "Photographic Production of Slides and Filmstrips." Kodak dealers have it for sale or can order it. It is particularly rich in details on attaining good quality color reproduction by the use of masking techniques. The danger that purchase of this 50¢ booklet turns out eventually to have been the first step toward establishment of a slide-and-filmstrip department in the organization with which the reader is affiliated, while slight, is undeniably present.

Arrhenius's clock



Time, as told by this clock, elapses at a rate that is temperature-dependent in the same way as the rate of a chemical reaction is temperature-dependent. Sensing is done by the thermistor probe, which goes into the reaction vessel. It's handy in a photographic lab because by time compensation it tames a ± 4 F temperature variation to the equivalent of ± 0.2 F control. Arrhenius, who wrote the equation but wasn't as hipped on photography as we are, would have been pleased.

Though we don't manufacture these for sale, we'll be glad to furnish a reprint that tells how we made ours. Drop a note to Eastman Kodak Company, Special Sensitized Goods Division, Rochester 4, N. Y.

Prices quoted are subject to change without notice.

Kodak
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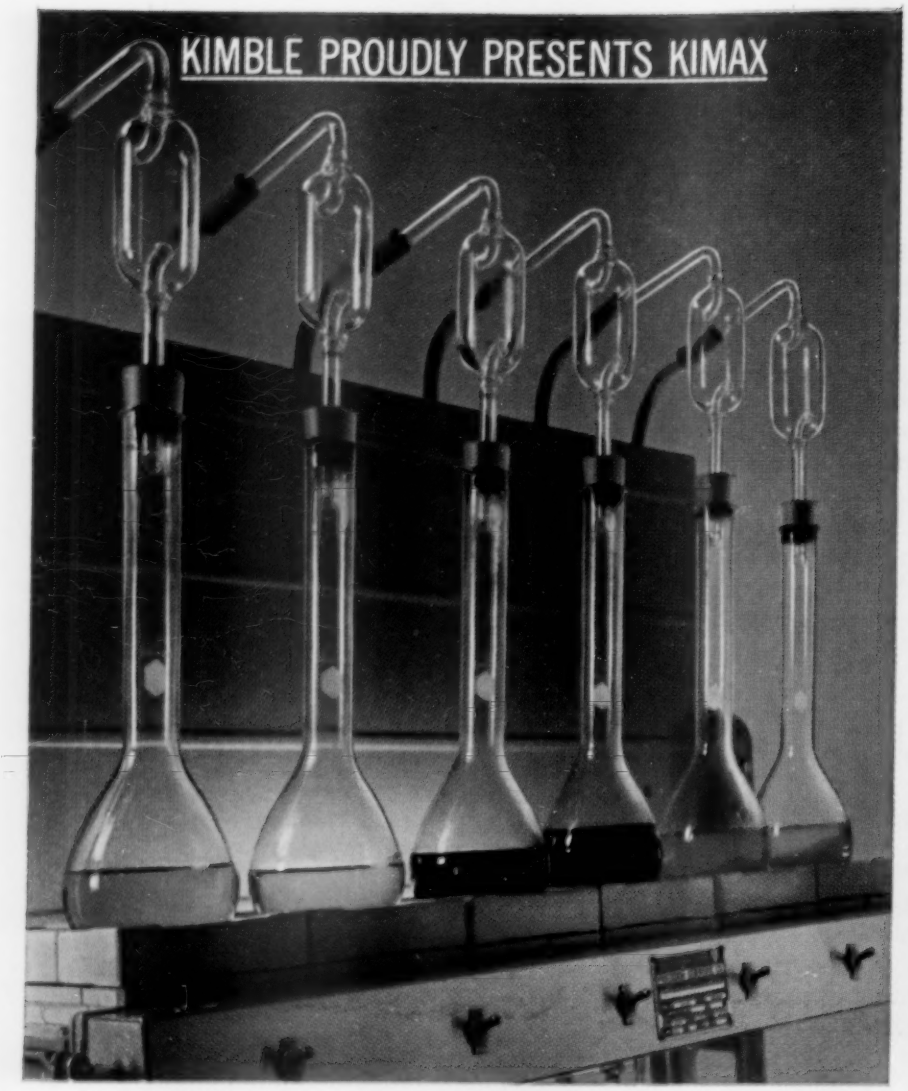
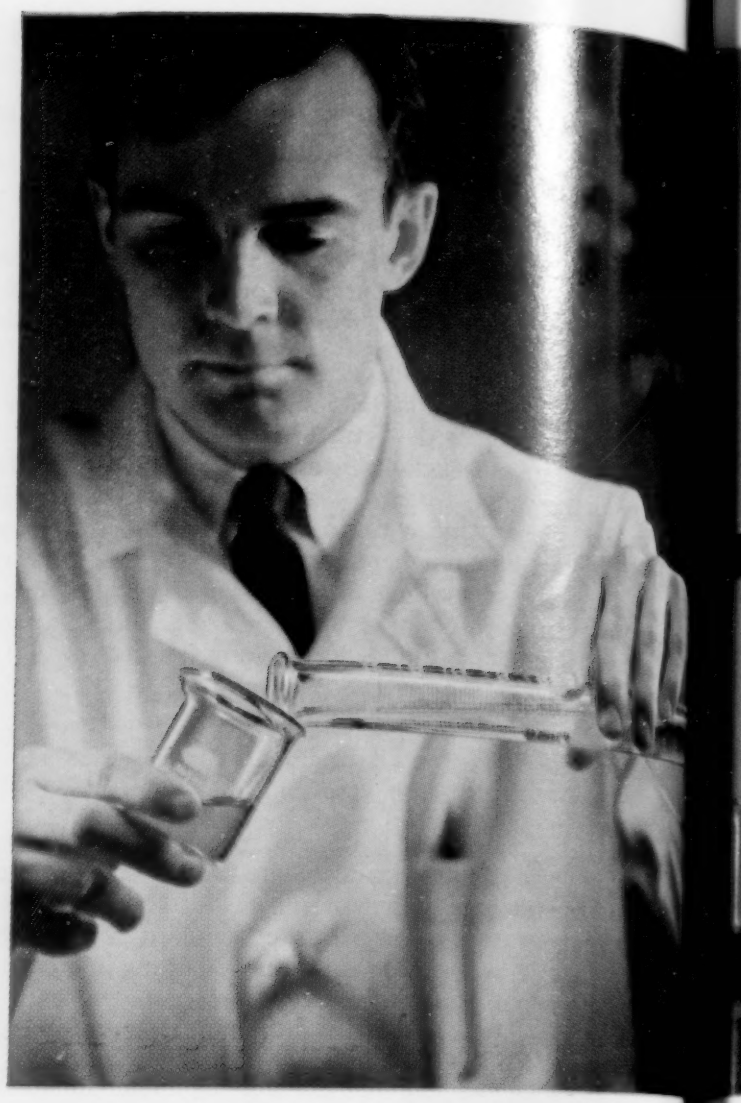
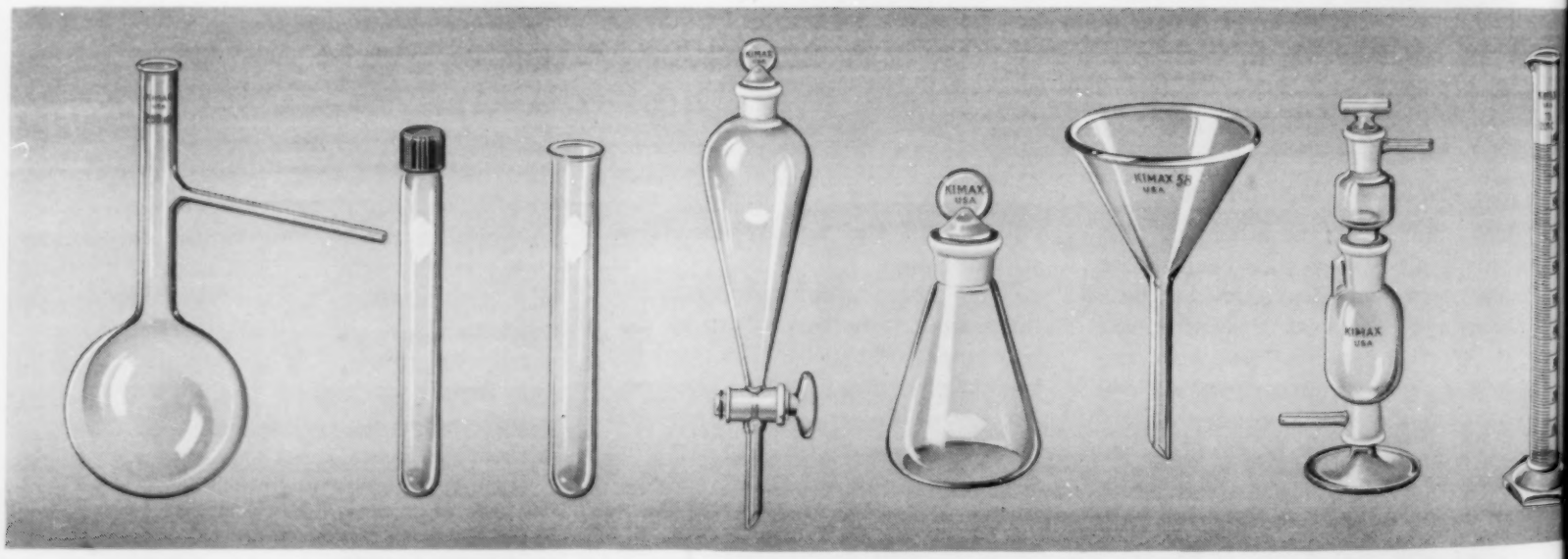
This is another advertisement where Eastman Kodak Company probes at random for mutual interests and occasionally a little revenue from those whose work has something to do with science





From corrosive alkali to biting acid



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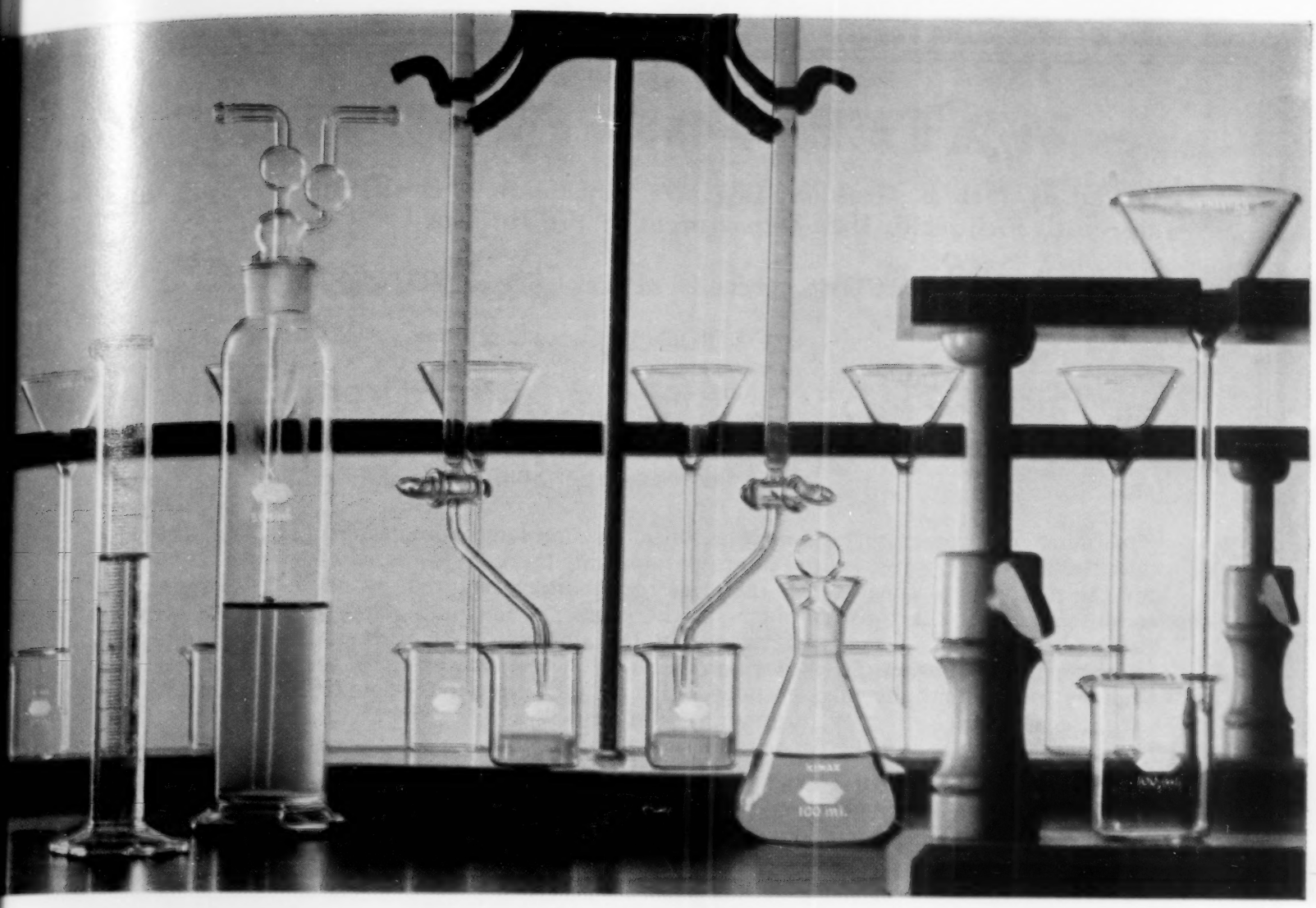
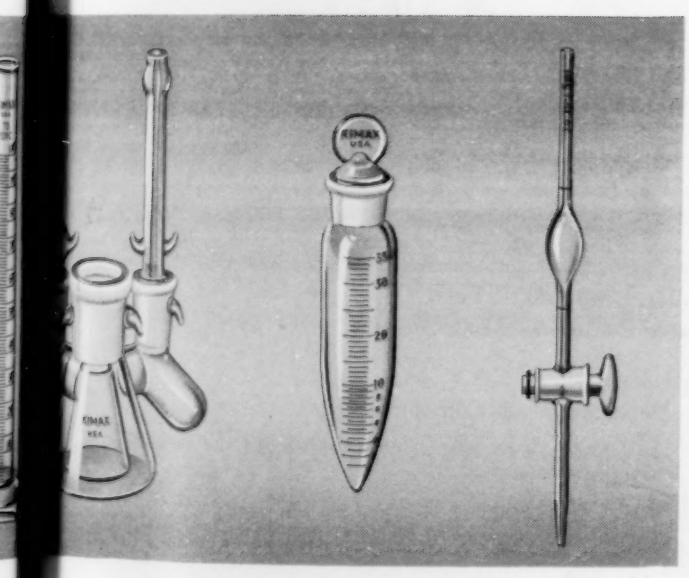
KIMAX is available through dealers in the United States, Canada and principal foreign cities.



id chemical attack means little to KIMAX®



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WATER FOR INDUSTRY

Edited by Jack B. Graham, Leggette, Brashears & Graham, New York, and Meredith F. Burrill, U.S. Department of the Interior.

6 x 9 inches, 141 pages, 18 figures, index, clothbound, 1956.

Price $3.75. AAAS Members' prepaid order price **$3.25.**

No. 45 in the symposium volume series of the American Association for the Advancement of Science.

Industrial productivity requires material resources and human ingenuity, and, of all the material resources, water is used in greater amounts than any other. It constitutes in bulk by far the major constituent of all material commodities required by industry. Sensing the serious nature of the water problem in the coming years, and its pertinence not only to national security but also to internal economic stability, the AAAS invited a panel of experts to present a symposium on *Water for Industry*. It was arranged by the AAAS Section on Geology and Geography, and cosponsored by the Sections on Engineering, Industrial Science, the Geological Society of America, the Association of American Geographers (New England Division), and the American Geophysical Union.

The eventual solution to the problem of water for industry will not involve industry alone, for water is a common property which properly serves not one but many users, and the attainment of peak efficiency of water will not be easily or quickly realized; but not to strive for this husbanding of a vital resource would be as damaging to our national well-being as for a person to ignore a wound and slowly bleed to death.

This book provides a perspective of present and impending water problems, and a wide audience—especially government, industry, geology and geography, and conservation groups—will find it valuable reading.

Contents

The Available Water Supply
C. G. Paulsen, United States Geological Survey

Water Requirements
H. E. Hudson, Jr., Hazen and Sawyer, and Janet Abu-Lughod, American Council to Improve Our Neighborhoods

Geographic Distribution of Manufacturing
Meredith F. Burrill, Office of Geography, U.S. Department of the Interior

Water and Steel: Fairless Works Water Supply
Ross L. Leffler, United States Steel Corporation

The Treatment and Disposal of Wastes in the Atomic Energy Industry
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Water Supply and Waste Disposal Requirements for Industry
Sheppard T. Powell, Consulting Engineer, and E. L. Knoedler, Sheppard T. Powell

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W. B. Hart, Pantech, Inc.

Correction of a Fluviatile Delinquent: The Schuylkill River
Francis A. Pitkin, Bureau of Community Development, Department of Commerce, Commonwealth of Pennsylvania

Water in the Future
J. Russell Whitaker, George Peabody College for Teachers

Discussion
Gilbert F. White, Department of Geography, University of Chicago
Felix E. Wormser, U.S. Department of the Interior

Also available: Volume #31 (paperbound, 1932), *Industrial Science — Present and Future*, $2.00

British Agents — Bailey Bros. & Swinfen, Ltd., Hyde House, West Central Street, London, W.C.1

American Association for the Advancement of Science

1515 Massachusetts Avenue, NW, Washington 5, D.C.

ceived the Mary Clark Thompson Medal "for his contribution to our knowledge of the biology and stratigraphic significance of the fossil brachiopods."

GEORGE VAN BIESBROECK, emeritus professor of astronomy, Yerkes Observatory, Williams Bay, Wis., received the James Craig Watson Medal "for his noteworthy contributions to astronomy."

ERNEST W. GOODPASTURE, scientific director, department of pathology, Armed Forces Institute of Pathology, Washington, D.C., received the Jessie Stevenson Kovalenko Medal "for outstanding contributions to medical science and for long and continued devotion to the study of his chosen field of pathology."

THEODOSIUS DOBZHANSKY, professor of zoology, Columbia University, received the Kimber Genetics Award as a "versatile and inspired student of the mechanism of heredity, and of the roles which genetic and environmental factors play in the origin and structure of populations and in the process of biological evolution."

PIERRE HUPE, Laboratoire de Géologie de la Sorbonne, Université de Paris, received the Charles Doolittle Walcott Medal "in recognition of his monumental work entitled 'Contribution à l'étude du Cambrien inférieur et du Précambrien III de l'Antiatlas marocain'."

WARREN C. JOHNSON has been elected vice president of the University of Chicago in charge of special scientific programs. He succeeds Walter Bartky, who died in March. The new vice president has been dean of the university's Division of the Physical Sciences since 1955 and a member of the faculty since 1927. He was made professor and chairman of the department of chemistry in 1945.

J. W. C. GATES, senior scientific officer, Light Division, National Physical Laboratory, Teddington, Middlesex, England, will be in the United States and Canada from 31 May to about 21 June. His itinerary probably will include: Washington (31 May–9 June); Boston; Ottawa, Canada; Rochester, N.Y.; Detroit; Harrodsburg, Ky.; Elgin Air Force Base, Fla.; and Pittsburgh.

W. E. VAN HEYNINGEN of Oxford University, a specialist in tetanus toxin, delivered the first annual John Howard Mueller memorial lecture at the Harvard Medical School on 24 April. Van Heyningen is a senior research officer of the Medical Research Council in the Sir William Dunn School of Pathology at Oxford. The lecture honors Mueller, who at the time of his death in 1954 was Charles Wilder professor of bacteriology and immunology at the Harvard Medical School.

ANDREW J. RAMSAY, professor of histology and embryology at the Jefferson Medical College, has been appointed professor of anatomy and head of the department at Jefferson. He is also director of the college's Daniel Baugh Institute of Anatomy. He succeeds the late George A. Bennett.

JOSEPH MORGAN, professor of physics and director of the engineering program at Texas Christian University, has been named chairman of the physics department, effective 1 September. He succeeds NEWTON GAINES, who is retiring after having been chairman for 34 years.

JOHN H. WILLIAMS, physicist at the University of Minnesota, has been appointed director of the Research Division of the U.S. Atomic Energy Commission. He was granted leave of absence by the university to accept the appointment. He succeeds THOMAS H. JOHNSON, who resigned on 1 October 1957, to manage the Research Division of the Raytheon Manufacturing Company, Waltham, Mass.

EUGENE G. ROCHOW, professor of inorganic chemistry at Harvard University, has been selected to present the annual Joseph J. Mattiello Memorial Lecture at the 36th annual meeting of the Federation of Paint and Varnish Production Clubs, which will take place in Cleveland, Ohio, 5–8 October.

HANS WELTIN, physicist, is affiliated temporarily with the U.S. Naval Radiological Defense Laboratory in San Francisco. In the fall he will become head of the physics department at Robert College, Istanbul, Turkey. Weltin has spent the past 7 years in the Orient on an assignment to teach U.S. military personnel at various stations in Japan, Guam, Korea, and Okinawa, for the U.S. Government and the University of California.

LELAND M. WHITE has been appointed director of research and development for the United States Rubber Company, replacing SIDNEY M. CADWELL, who has retired after 39 years of service. White joined the company's research and development department in 1940 as a research chemist after obtaining his Ph.D. degree in physical chemistry and physics from the University of Kansas. He rose to research group leader, then department head, and since 1953 has been assistant director of the department.

Cadwell, director of research and development since 1946, is the holder of 59 patents. Among them are antioxidants which add to the service life of many rubber products, including tires, and a tough cover for golf balls known as the "Cadwell cover." Cadwell was among the first to recognize the advantages of using butyl rubber for inner tubes and rayon cord for tires.

HENRY I. WOHL, formerly head of the agricultural department of St. Martin's College, Olympia, Wash., has joined the Jackson B. Hester Agricultural Research Laboratories, Elkton, Md.

Recent Deaths

FANNIE L. DUHRING, Philadelphia, Pa.; 80; bacteriologist and former curator of animals at the Wistar Institute of Anatomy at the University of Pennsylvania; 14 Apr.

AURELIANO M. FERNANDES, Lisbon, Portugal; 73; mathematician who retired in 1954 as professor of mathematics at the Technical University of Lisbon; 19 Apr.

HENRY J. FRANKLIN, Wareham, Mass.; 75; entomologist whose pioneering research led to the control of two pests that almost destroyed the Massachusetts cranberry industry in 1905; founded the Massachusetts Experimental Cranberry Station in East Wareham in 1908; 16 Apr.

ROSALIND FRANKLIN, London, England; 37; specialist in virus structure; internationally known for work on the structure of nucleoproteins in relation to virus diseases and genetics; 17 Apr.

HUGH A. KUHN, Chicago, Ill.; 63; physician who lectured at European universities; president of the American Society of Ophthalmology in 1952 and vice president of the American College of Allergists in 1957; 17 Apr.

E. F. LISKUN, Moscow, U.S.S.R.; 84; academician and specialist in animal husbandry; head of the faculty at the Timiryazev Agricultural Academy in Moscow; published more than 700 works; 20 Apr.

WILLIAM L. RAWS, Melbourne, Australia; 79; chairman of the board of the Imperial Chemical Industries of Australia and New Zealand, Ltd.; 20 Apr.

JOHN E. SNOW, Athens, Ohio; 92; professor emeritus of electrical power production at Illinois Institute of Technology in Chicago; 19 Apr.

GABRIEL TUCKER, Philadelphia, Pa.; 77; emeritus professor of bronchoesophagology and laryngeal surgery at the University of Pennsylvania's Graduate School of Medicine; specialist in the removal of lung cancers and the use of the bronchoscope; 17 Apr.

SAMUEL A. VEST, Charlottesville, Va.; 53; chairman of the urology department at the University of Virginia Medical School; 6 Apr.

Book Reviews

What's Happened to Our High Schools? John F. Latimer. Public Affairs Press, Washington, D.C., 1958. vii + 196 pp. $3.75.

This question is not only asked but answered by John F. Latimer in a factual, even-tempered, and scholarly analysis. It is a question that has been asked many times by many people in the course of the present decade. From the critics of our schools and their product has come the deep-throated chorus, "They're worse!" And from the educators has come the high-pitched cry, "They're better!" The argument has virtually reached the 'tis-'tain't intellectual level, as each side eloquently quotes Scripture—and statistics—to prove its points.

Latimer's searching, historical analysis of our changing school system represents the results of four years of painstaking research. His facts and figures will end many a statistical argument, and they certainly demonstrate the fact that our schools are different and that their curriculum has changed drastically—whether for better or for worse may still be open to argument, but on some of the author's thoughtful conclusions it is hoped there will be increasing agreement.

Critics of the high-school curriculum have claimed that only 45 percent of our high-school pupils take work in mathematics. This claim is countered by Harold C. Hand, professor of education at the University of Illinois, with the contention that "only about 5 percent of our high school youngsters are not taking any work in mathematics these days." Using the same source of information as Hand, Latimer finds that the total percentage of students taking mathematics is 55.0. Again, there has been the argument about how many of our high schools offer courses in physics and chemistry. This question, as a matter of fact, is unimportant. What matters is how many students are taking physics and chemistry. Latimer finds that in 1949 only 5.4 percent of the high-school students in grades nine to twelve were enrolled in physics, and only 7.6 percent in chemistry. Even if these figures for a single year are multiplied by four—a dubious procedure, at best—the totals are not impressive, and they represent substantial drops from the percentages of students enrolled in these subjects from 1890 to 1910. Meanwhile, physical geography and geology have virtually dropped out of the high-school curriculum, whereas biology has shown a spectacular rise, and general science an even more phenomenal increase—from 0 in 1910 to 20.8 percent in 1949.

In general, Latimer's story is one of rise and fall for the "hard-core" subjects—mathematics, science, and foreign languages (English appears to have held its own fairly well)—and a sharp increase in social studies, business education, vocational and related subjects, home economics, and physical education.

Thoughtfully, Latimer concludes: "It is no accident that the main core of those subjects without which life in the modern world is inconceivable is the same in all civilized countries: Mathematics, science, foreign languages, history, and one's own native tongue. The logic and principles of mathematics, the laws of science, the lessons of history are the same, no matter in what language they are studied and learned. Native tongues differ, but each is the gateway to its own procreation of culture, thought, and communication. Foreign languages differ, but they constitute the media for the transmission of ideas and for the cross-fertilization of cultures. These are the subjects around which coordinated courses of study for all students, regardless of ability, should be built, from the grades through high school. . . ."

Latimer demolishes what he calls "split-level" education—easy subjects for the slow and hard subjects for the bright. Again, to use his own words:

"That some students are not capable of strenuous mental effort is unfortunately true. The proper solution for the slow-learner is not the notion of 'easier' subject-matter. Tremendous as the task will be, it lies rather in the development and use of special texts and teaching methods and in grouping according to ability, that will give the less gifted an education that will differ in *quantity, not in kind,* from that of his more gifted fellows. This is the essence of democratic education, the opportunity to learn, at one's own pace and ability, the basic principles of those subjects without which life in the modern world is inconceivable.

"But split-level education often results, not from lack of ability, but from *underestimation* of a student's capacity, by himself, his parents, or his advisor, or from the very human tendency to which even students are prone, to follow the path of least resistance. Those who make their own choice in high school, whatever the reason, often realize it too late to make the necessary substitutions or to change their objectives. The effect on the student can be disastrous, in lowered morale and self-confidence, in loss of interest, and in lack of adequate preparation for college or for the larger world outside. Here is the educational wasteland we can and must reclaim."

While the Congress and the public are so keenly concerned about our educational goals, the study of John F. Latimer's book is a "must." As he says: "We must face these facts realistically . . . these data make it possible for the reader to draw his own conclusions, in agreement or disagreement with those suggested by the writer. . . . The use of facts and figures is not meant to imply that the *quality* of education is measured by statistics. But by means of these facts and figures, cold and lifeless as they may seem, we may be able to take the educational pulse of America and prescribe with confidence for her educational health in the years to come."

HOWARD A. MEYERHOFF
Scientific Manpower Commission, Washington, D.C.

Antiseptics, Disinfectants, Fungicides, and Chemical and Physical Sterilization. G. F. Reddish, Ed. Lea and Febiger, Philadelphia, ed. 2, 1957. 975 pp. Illus. $15.

With its appearance in 1954, this volume became a standard reference work on the subject of applied disinfection and sterilization. The publication of a second edition within a relatively short time is indicative of the active interest of the editor and his 27 collaborators in maintaining this book as an up-to-date reference standard in this area.

Like the first, this second edition is concerned for the most part with the practical aspects of chemical antisepsis, disinfection and sterilization, and heat sterilization of canned foods and similar products. Antimicrobial compounds, other than the chemotherapeutic drugs, are considered in detail, this edition including new material on the extensively studied phenolic compounds and on the use of antibiotics in food preservation and for other purposes and completely new chapters on sterility testing, by Brewer, and on the applications of ultraviolet radiation, by Schechmeister. The outstanding chapter, by Spaulding, on chemical disinfection of surgical instru-

ments has been made even more comprehensive.

The editor has wisely chosen to accept a certain amount of repetition to make possible a more rounded presentation of relevant material in the subdivisions of the text so that each tends to stand alone, adding to reference value. At the same time, other than the chapter by Wyss concerned with natural and acquired resistance to antimicrobial substances, there is no general theoretical discussion of microbial growth inhibition and death, or consideration of the implications of the dynamics of the processes of disinfection. Possibly the authors assume an adequate background on the part of the reader, and this assumption is doubtless justified, but in my opinion the book would gain in stature by presentation of the subject material against an authoritative theoretical discussion.

WILLIAM BURROWS
Department of Microbiology,
University of Chicago

Handbook of Chemical Data. F. W. Atack, Ed. Reinhold, New York, 1957. 629 pp. $6.75.

This is a handy little pocket-size volume of essential chemical information in tabular form. Although by no means as complete as the familiar *Lange* and *Chemical Rubber* Handbooks, it lists the properties of 2100 inorganic and 5500 organic compounds and contains useful tables of specific gravities of solutions, logarithms, formulas, gravimetric factors, and so on. The book is small enough to fit a pocket or a brief case, the typeface is easy to read, and the arrangement of headings makes it possible to find information quickly without using the index.

Psychology, Evolution and Sex. Cecil P. Martin. Thomas, Springfield, Illinois, 1957. x + 166 pp.

Evidence for any biological theory is never complete. Destructive criticism points out the gaps in the evidence without offering a better explanation of the demonstrated order of facts. Constructive criticism offers an alternative theory that is more in accord with the evidence, or provides new facts that logically force modification or overthrow of inadequate theories.

Martin's book attempts to refute the "mutation-selection theory" of evolution, and, in its place, he offers the alternative theory which is little more than a modern restatement of Lamarckian inheritance of acquired somatic characters—psychological, physiological, and structural. He does not claim proof for Lamarckism, but he is strong in his opinion that the "mutation-selection" theory is less well established. He does not incorporate adequately the advancing information on the roles of recombination, inbreeding, and population genetics. He thus fails to understand how complex functions or regressions can evolve in conformity with modern genetic and ecologic principles. Many sweeping statements are made in contradiction to available evidence not mentioned in his limited bibliography. For example, he says (page 23): "That the modificatory preferences become, in time, hereditary seems fairly certain. That they do so through mutation and natural selection has not been proved and appears to be virtually impossible." He seems sincere in his viewpoint and convictions but, in my opinion, too often substitutes biased generalizations for a careful analysis of the facts, pro and con. When he does gather facts together, he often leaves out evidence opposed to his conclusions. It would take far too much time and space to supply the data that would tend to refute his major conclusions, but I suggest that the interested reader examine the following books and papers: (i) W. C. Allee, A. E. Emerson, O. Park, T. Park, and K. P. Schmidt, *Principles of Animal Ecology* (Saunders, 1949); (ii) G. L. Stebbins, Jr., *Variation and Evolution in Plants* (Columbia University Press, 1950); (iii) T. Dobzhansky, *Genetics and the Origin of Species* (Columbia University Press, ed. 3, 1951); (iv) J. Huxley, A. C. Hardy, and E. B. Ford, *Evolution as a Process* (Allen and Unwin, 1954); (v) H. J. Muller, "Life," *Science* **121**, 1 (1955).

These publications give both discussions of concepts and many pertinent bibliographical references that will supply much of the critical data lacking in Martin's book. Of course, these references do not solve completely all of the problems raised, and there are some differences of opinion and interpretation among the active investigators of evolutionary dynamics, but I think the major questions raised by Martin are largely answered. Old-fashioned Lamarckism is now rather thoroughly discredited. I would also suggest that "The evolution of adaptations" by C. H. Waddington [Endeavour **12**, 134 (1953)] be read. This essay shows how examples once used by Lamarckians may be harmonized with modern genetic and ecologic theory. Instead of a cause always preceding an effect, genetic substitution indicates that the effect, in a sense, causes the selection of genetic triggers setting off processes once physiologically acquired. In time there may be an evolutionary feed-back from effect to cause, provided only that the cause is continuous or repeated.

It is well to have a healthy skepticism concerning any theory, major or minor. Science grows by the accumulation of new evidence and the refutation or verification of theories explaining the order of facts. Relationships are observable facts as much as are the facts that are related. However, in the book under review, I am not convinced that the alternative of Lamarckism is justified by the evidence, nor am I convinced that Martin has penetrated thoroughly enough into the evidence that supports the theory he attempts to refute. He accuses others of approaching the problem with fixed preconceptions and dogmatic attitudes, but it seems to me that Martin may have erred by proceeding from a somewhat subjective bias himself. Unfortunately none of us can be thoroughly objective and remain human.

ALFRED E. EMERSON
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Introduction to the Mechanics of Stellar Systems. Rudolf Kurth. Pergamon Press, New York and London, 1957. ix + 174 pp. $9.

A stellar system may be characterized as an assemblage of mass points, each moving under the combined gravitational influence of all the others, with no spatial bound on the motion of any individual particle. No straightforward theory has ever been developed for the mechanics of such a system; instead, techniques have been drawn in catch-as-catch-can fashion from theories of the *n*-body problem, the motion of continuous media, statistical mechanics, and kinetic theory. Rudolf Kurth says he has attempted to bring out the essential parts of this subject, but the scope of his book is not as broad as the title would indicate. He concerns himself chiefly with the abstract mechanical principles that may be applied to stellar systems. He cannot claim to give a systematic presentation of current theories of stellar dynamics and their application to actual stellar systems.

The book begins with a short summary of observed characteristics of existing stellar systems, followed by a consideration of the basic assumptions and methods on which a theory of their mechanics may be based. Next comes a discussion of the dynamics of many-particle systems. The two central chapters deal successively with stellar systems as assemblies of gravitating mass points and as gravitating continua. The book closes with a brief discussion of the relation of statistical mechanics to the mechanics of stellar systems.

The author's point of view is abstract and general. Such an approach can be very powerful, but in this book it is not.

Many discussions end with vague generalities, solutions-in-principle, or doubts as to whether the mathematical model really applies. Moreover, one may question the choice of material and the emphasis. Kurth spends 12 pages on a proof of Poincaré's recurrence theorem, in spite of the fact that the escape of stars is known to make the evolution of stellar systems irreversible. On the other hand, the theory of stellar encounters is completely omitted, along with its evolutionary consequences and its significance for the basic approach to stellar dynamics. The occasional illustrative use of observational data shows no appreciation of their significance—and especially their uncertainties and discrepancies. Indeed, the author confesses that most of his data are taken not from the original sources but from a general text published in 1950.

The foregoing evaluation reflects in part my own prejudice. A general theory need not set forth its own applications, but it should go so far as to ease any doubts as to whether the applications can be made in practice.

IVAN KING
University of Illinois Observatory

Instinctive Behavior. The development of a modern concept. Translated and edited by Claire H. Schiller. International Universities Press, New York, 1957. xix + 328 pp. Illus. $7.50.

Claire Schiller has translated from the German eight papers illustrative of European studies of animal behavior during the 1930's. Originally published in scattered sources, these have not been readily available to American readers. An introduction by K. S. Lashley and a preface by N. Tinbergen help to orient the reader who is not already acquainted with the ethological movement.

The introductory paper by J. von Uexküll, "A stroll through the worlds of animals and men," stands apart from the remainder of the book. It is pleasant reading; however, the perceptual worlds described are not accessible to scientific investigation. Six papers are concerned with the observational and theoretical foundations of ethology, largely in the words of Tinbergen and Konrad Lorenz. The bringing together of papers originally presented separately has resulted in undue repetition of such theoretical points as the distinction between "instinct" and "taxis" and the definition of "innate releasing mechanisms." Judicious editing might have provided more space for observational and experimental material. However, by the time the reader has encountered Lorenz's description of an instinct for the third time, he has learned that ethologists, too, reject the concept of an instinct as a goal-directed chain of activities. So wide is the gap in meaning between Lorenz's "instincts" and McDougall's "instincts" that a better word should be found. P. H. Schiller is represented by a hitherto unpublished study of manipulative play in young chimpanzees. He throws doubt on the importance of "insight" as a factor in tool-using by these primates.

Ethologists are concerned with precise descriptions of responses, with their adaptive value, and with their phyletic evolution—problems which receive little consideration in contemporary comparative psychology in America. Fundamentally trivial responses such as bar-pressing or running down an alley serve well enough to establish quantitative relationships between antecedent events and behavior. The law, not the response, is important. Nevertheless, I was struck by a resemblance between the ideas of Lorenz and Schiller on the interrelationship of instincts and learning and B. F. Skinner's concept of the conditioning of operant behavior. Then, on page 286, I found that the comparison had already occurred to Schiller. Perhaps the entire range of ethological theory could be reviewed from this point of view.

The usefulness of the volume is enhanced by a bibliography of ethological studies, including some as late as 1955. Some of the newer research is more quantitative, techniques having been borrowed from other experimental sciences. These early papers are, however, important reading for the animal behaviorist interested in the history of ideas.

JOHN L. FULLER
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New Books

Methyl Glucoside. Preparation, physical constants, derivatives. G. N. Bollenback. Academic Press, New York, 1958. 188 pp. $5.50.

The Alimentary Tract of the Ruminant. David Benzie and A. T. Phillipson. Thomas, Springfield, Ill., 1957. 24 pp. and 54 plates. $5.50.

Application of Tensor Analysis. A. J. McConnell. Dover, New York, 1957 (originally published as *Applications of the Absolute Differential Calculus*). 330 pp. Paper, $1.85.

Block Diagrams. And other graphic methods used in geology and geography. Armin Kohl Lobeck. Emerson-Trussel, Amherst, Mass., ed. 2, 1958. 221 pp. $6.

Biochemical Investigations. In diagnosis and treatment. John D. N. Nabarro. Little, Brown, Boston, ed. 2, 1958. 310 pp. $6.

The Medical World of the Eighteenth Century. Lester S. King. University of Chicago Press, Chicago, 1958. 366 pp. $5.75.

The Guide to Garden Flowers. Their identity and culture. Norman Taylor. Houghton Mifflin, Boston, 1958. 332 pp. $4.95.

Antony van Leeuwenhoek and His "Little Animals": Being Some Account of the Father of Protozoology & Bacteriology and His Multifarious Discoveries in These Disciplines. Collected, translated and edited, from his printed works, unpublished manuscripts, and contemporary records by Clifford Dobell. Russell and Russell, New York, 1958. 442 pp. $10.

Bibliography and Index of Geology Exclusive of North America. vol. 21. Marie Siegrist, Mary C. Grier *et al.* Geological Society of America, New York, 1958. 845 pp.

Principles of Biology. W. Gordon Whaley, Osmond P. Breland, Charles Heimsch, Austin Phelps, A. R. Schrank *et al.* Harper, New York, ed. 2, 1958. 887 pp. $6.75.

Oral Communication of Technical Information. Robert S. Casey. Reinhold, New York; Chapman & Hall, London, 1958. 199 pp. $4.50.

The Story Behind the Word. Some interesting origins of medical terms. Harry Wain. Thomas, Springfield, Ill., 1958. 350 pp. $8.50.

Social Class and Mental Illness. A community study. August B. Hollingshead and Frederick C. Redlich. Wiley, New York; Chapman & Hall, London, 1958. 454 pp. $7.50.

The Future Supply of Oil and Gas. A study of the availability of crude oil, natural gas, and natural gas liquids in the United States in the period through 1975. Bruce C. Netschert. Johns Hopkins Press, Baltimore, 1958. 145 pp. $3.

The Measurement of Color. W. D. Wright. Macmillan, New York, 1958. 272 pp. $10.75.

Vibration and Impact. Ralph Burton. Addison-Wesley, Reading, Mass., 1958. 320 pp. $8.50.

Your Speech Reveals Your Personality. Dominick A. Barbara. Thomas, Springfield, Ill., 1958. 189 pp. $5.50.

Ion Exchange Resins. Robert Kunin. Wiley, New York; Chapman & Hall, London, ed. 2, 1958. 479 pp. $11.

Theoretical Physics. Thermodynamics, electromagnetism, waves, and particles. F. Woodbridge Constant. Addison-Wesley, Reading, Mass., 1958. 377 pp. $7.50.

Chromatographic Techniques. Clinical and biochemical applications. Ivor Smith. Heinemann, London; Interscience, New York, 1958. 322 pp. $6.75.

Pears Cyclopaedia. An everyday work of reference for the home, office, and school. L. Mary Barker, Ed. Pears, Isleworth, England, ed. 66, 1957 and 1958. 975 pp. 15s.

Biological Ultrastructure. Arne Engstrom and J. B. Finean. Academic Press, New York, 1958. 335 pp. $8.

5-Hydroxytryptamine. Proceedings of a symposium held in London on 1–2 April 1957. G. P. Lewis, Ed. Pergamon Press, New York and London, 1958. 270 pp. $9.50.

Fundamental Concepts of Inorganic Chemistry. Esmarch S. Gilreath. McGraw-Hill, New York, 1958. 428 pp. $7.50.

Reports

Do the Nurse Honey Bees Recognize the Sex of the Larvae?

The queen in the honey-bee colony lays two types of eggs, fertilized and unfertilized. Usually the fertilized eggs are laid in the smaller (worker) cells or in the queen cells and produce either workers or queens. The unfertilized eggs are deposited in larger cells and produce drones. The food which the nurse bees feed to queen larvae is almost devoid of pollen; a little pollen is present in the food of older worker larvae, but the food of older drone larvae contains considerable amounts of pollen.

Planta (*1*) did not find any pollen in the food of older worker larvae. However, the food of drone larvae, over 4 days old, showed a great admixture of pollen grains (15,000 grains in 1 mg of food). On microscopical examination, Haydak and Vivino (*2*) found 9 to 11 grains of pollen per field of vision in the food of older worker larvae, and Haydak (*3*) counted an average of 38 grains in the food of older drone larvae. It appears that the nurse bees differentiate between the drone and the worker larvae. Is the sex of the larvae or is the size of the cells instrumental in this differentiation? Gontarski (*4*) considers that not the cell content (the type of the larvae) but the form and the size of the cells are the stimuli determining the type of food deposited in the cells by the nurse bees.

It is a known fact (*5*) that, when offered only drone combs, the queen will lay fertilized eggs in the drone cells. In the presence of worker combs, the queen begins to lay normally, depositing fertilized eggs in the worker, and unfertilized in the drone, cells. On the basis of this knowledge, the following experiment (*6*) was designed.

In the spring (9 May) two queenright packages were hived on drone combs and drone foundation. The queen started egg laying, and the larvae were fed normally. The food was then taken from the cells that contained the older larvae. The lower walls of the cells were destroyed, the larvae were removed, and the food was taken out with the help of a royal-jelly spoon. Usually five vials were used, the food of ten cells being placed in each. The content of each vial was thoroughly mixed. Five samples were taken from each vial, and five readings were made on each sample, under an objective lens of ×44 magnification. The average pollen count per field of vision was 12 grains.

The larvae were sealed by the bees, with worker-shape cappings, and worker pupae were found in the cells. Ten days later a second set of samples was taken, from the drone cells that contained worker larvae, and the average pollen count was three grains of pollen per field of vision. At this time, about half of the drone combs were taken out, and worker combs were added to the colonies. When older worker larvae were found in the worker cells, the samples of food of older larvae were taken from the worker cells and from the drone cells containing worker larvae. The average pollen count was four grains of pollen for the food of older worker larvae from the drone cells and five grains of pollen for that from the worker cells containing older worker larvae. When the colony started to rear drone larvae in the drone combs, the samples of food from the cells that contained older drone larvae averaged 16 grains per field of vision.

The queens were removed from the colonies. When laying workers appeared, samples of the food from the drone cells that contained older drone larvae were taken again. The average count was six grains of pollen per field of vision. The food from the queen cells built over the drone larvae (which changed to drone pupae in the constant temperature chamber) contained 0.3 grain of pollen per field of vision.

The results seem to indicate that, at the beginning, the bees hesitated somewhat in recognizing the sex of the larvae in the drone cells, thus supplying the older worker larvae with a larger amount of pollen. However, later, they were giving the food for worker larvae, containing less pollen, to the older worker larvae reared in both the drone and the worker cells. When the unfertilized eggs were laid in the drone cells, the bees recognized the drone larvae and fed the older larvae a ration that contained increased amounts of pollen, as is done normally. This would indicate that the nurse bees, under normal conditions, recognized the sex of the larvae irrespective of the size of the cells.

An entirely different picture was observed when the colonies became hopelessly queenless and the laying workers began their activity. In this case the older drone larvae in the drone cells were fed the food containing less pollen, which is normally offered to the older worker larvae. Moreover, the drone larvae in the queen cells received the royal jelly which is given to normal queen larvae. Thus it appears that, in a laying workers' colony, the bees did not differentiate with respect to the sex of the larvae. The cause of this phenomenon is difficult to explain at present.

From these findings it appears that, in the queenright colonies, the nurse bees recognize the sex of the larvae and feed the older larvae of both sexes accordingly. However, in the hopelessly queenless colonies, it seems that the nurse bees feed the older drone larvae as if they were female larvae.

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References and Notes

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3. M. H. Haydak, *ibid.* **50**, 73 (1957).
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6. This report is paper No. 3876 in the Scientific Journal Series, Minnesota Agricultural Experiment Station, St. Paul.

23 December 1957

Upstream Bottom Currents in New York Harbor

Analysis of data obtained during the 1952 current surveys in New York Harbor by the Coast and Geodetic Survey reveal the net upstream movement of large volumes of water near the bottom. These results were possible because the accurate determination of the flood and ebb currents, made it possible to calculate the flow of the nontidal or residual currents flowing in the same direction. This report explains the method whereby

All technical papers are published in this section. Manuscripts should be typed double-spaced and be submitted in duplicate. In length, they should be limited to the equivalent of 1200 words; this includes the space occupied by illustrative or tabular material, references and notes, and the author(s)' name(s) and affiliation(s). Illustrative material should be limited to one table *or* one figure. All explanatory notes, including acknowledgments and authorization for publication, and literature references are to be numbered consecutively, keyed into the text proper, and placed at the end of the article under the heading "References and Notes." For fuller details see "Suggestions to Contributors" in *Science* **125**, 16 (4 Jan. 1957).

the volume of flow for nontidal currents was calculated, discusses the upstream flow near the bottom, and shows how variations in fresh-water inflow may be inferred from current measurements alone.

Current measurements were made with the Roberts radio current meter (*1*). Observations of current velocity and direction were recorded every half hour, usually for at least 100 hours, at each of 39 stations, with meters suspended generally at one-fourth, one-half, and three-fourths the charted water depth. This report concerns only the stations in four profiles across New York Harbor and the Hudson River. The Sandy Hook profile consisted of seven stations across the mouth of the harbor from Sandy Hook to Rockaway Point. The Governors Island profile consisted of seven stations across the harbor south of the tip of Manhattan and 12 miles from the harbor mouth. The Riverdale and West Point profiles, of three stations each, were located in the Hudson River, 27 and 56 miles, respectively, upstream from the harbor mouth.

Reversing tidal currents flowed upstream (flood) and downstream (ebb) past each meter, reversing direction four times each tidal day (24 hr, 50 min). The net movement of water throughout a tidal cycle was determined at each meter location as follows: current curves were constructed in which the half-hourly velocities over approximately 100 hours were plotted as ordinates and times were plotted as abscissas. These curves were reduced to obtain mean values for flood and ebb velocities at strength, and for flood and ebb durations. Since the current velocity curve approximates a cosine curve, the mean value of all ordinates within any flood or ebb cycle is equal to $2/\pi$ (or 0.637) times the maximum ordinate of the curve. Thus the mean velocity of the current throughout the flood and ebb periods was taken as 0.637 times the velocity at strength. Multiplying the mean flood or ebb velocity by the duration of flood or ebb gave mean values, over the observation period, for the flood excursion and the ebb excursion, and the difference between these two values indicated the net movement per tidal cycle at that meter. This value was determined for each meter in each of the four cross sections. Each cross section was drawn to scale, and the area of the plane of the cross section represented by each meter was determined by drawing grid lines midway between adjacent meters. This area was multiplied by the net movement per tidal cycle to give the net upstream or downstream volume moved through each segment of each cross section during one tidal cycle. These calculations are based on the assumption that all the water flowing through each segment flows at the rate measured by the meter within that segment. The results are given in Table 1.

Table 1. Volume of flow measured in each of four cross sections. Upstream and downstream values indicate the sums of all segments showing net movement in those directions.

Volume of flow (1000 ft³/sec)				
Upstream	Downstream	Net downstream	Date of survey (1952)	Mean date
		Sandy Hook		
17.0	78.0	61.0	5/28–6/7	6/2
		Governors Island		
19.1	70.1	51.0	6/10–6/23	6/16
		Riverdale		
5.9	40.8	34.9	5/24–5/28	5/26
		West Point		
2.1	20.0	17.9	5/20	
0.0	34.7	34.7	5/21	
0.0	38.9	38.9	5/22	
0.0	51.5	51.5	5/23	

The Sandy Hook cross section showed that, during the observations, nontidal currents flowed in opposite directions in different portions of the cross section. The downstream movement was concentrated in the upper central portions of the stream, whereas the upstream current was noted at the bottom meter at all but the shallowest of the seven stations in this section. The two end stations in the section showed net up-harbor movement also at the surface, reflecting a localization of the surface flood currents. Nontidal upstream flow amounted to 17,000 ft³/sec. At the Governors Island cross section, the nontidal upstream bottom current was found only at the two deepest meters, and the net upstream bottom flow was 19,100 ft³/sec. An upstream bottom current was also found 27 miles upriver in the Riverdale cross section. Here, however, the bottom flow was only 5900 ft³/sec, again at the two deepest meters. At West Point, 29 miles farther up the Hudson, a net upstream bottom current of only 2100 ft³/sec was noted the first day of the series, and net movement was downstream at all depths on the following three days.

The water brought into the harbor along the bottom obviously is not accumulating there, so it must be mixing with the overriding, outflowing Hudson River water and returning to the sea. The mechanism whereby sea water is "pumped" in along the bottom of an estuary has been described experimentally (*2*) and has been observed in other estuaries (*3*). In the present case, however, no salinity measurements were made, and the calculations were possible only because of the completeness of the current surveys.

Since the inflowing bottom water must be returned, then comparable volumes must also be flowing out, mixed with the overriding river water, and the difference between the upstream and downstream flow must therefore be a measure of the river flow. Table 1 shows that this difference was not uniform during the surveys and suggests that there were large changes in the volume of river flow. At three of the four profiles, stations were not all observed simultaneously but rather in two separate groups, so that values for the net downstream movement must be referred to the mean time of the observations. At West Point, however, all stations were observed each day for four days, so that daily values for the net downstream movement could be computed. These were compared with the total daily volume of river flow at the eight Geological Survey gaging stations which measure the flow that eventually passes through the West Point section (*4*). The river flow at West Point, computed from the current observations, showed a steady increase during the four days 20 to 23 May to a high of 51,500 ft³/sec (Table 1). This most closely resembles an increase at the gaging stations during the four days 10 to 13 May to a high of 56,700 ft³/sec (*4*), indicating a lag of 10 days between gaging and the flow past West Point. Seventy-three percent of the water flowing through this profile was measured at the Green Island gaging station 95 miles farther upriver, indicating a rate of advance of 9.5 miles per day for the largest part of the flow. A comparison of the gaging station records with the computed net downstream flow at the mean time of observation of the other three sections suggests lags of 10 to 11 days at the Riverdale section and 20 to 21 days for the Sandy Hook and Governors Island sections. These latter values are approximate at best, because mean times of observations had to be used, but they accord well with the reported delay of 20 days for the adjustment to reach the upper harbor, reported by Ayres (*5*) on the basis of determinations of the fresh water fraction made by salinity measurements.

Additional surveys of the currents and salinity in New York harbor by the U.S. Coast and Geodetic Survey this year should provide data which will delineate and explain the upstream bottom currents more fully and which will make it possible to determine more accurately the role of river discharge and the effects of the East River not considered here.

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29 January 1958

Effect of Trypsin Inhibitor on Passage of Insulin Across the Intestinal Barrier

The finding of trypsin inhibitor in colostrum led to the hypothesis that the physiological role of the inhibitor is to protect the antibodies of colostrum from being digested and thus to facilitate their absorption (*1*). Some circumstantial evidence confirming this hypothesis has been accumulated (*2, 3*). For a direct experimental assault, insulin was chosen as the test protein, because its passage into the blood stream is reflected by the blood sugar level.

Early attempts to administer insulin through the gastrointestinal tract have been reviewed by Jensen (*4*). It is interesting to note that Murlin and Hawley (*5*) and Eaton and Murlin (*6*) used blood plasma as a source of "antitrypsin," whereas Harned and Nash (*7*) used an extract from *Ascaris*. The quantities of the inhibitor present in such preparations were, however, much lower than those used now. The maximal positive effect reported was a temporary disappearance of glycosuria in depancreatized dogs, with (*6*) or without a significant (*7*) lowering of the blood sugar level.

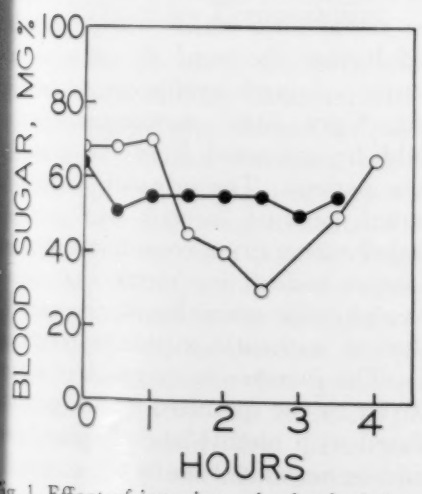

Fig. 1. Effect of intraintestinal administration of insulin on the blood sugar level. Open circles, experiments in which 6 units of insulin (40 units/kg) plus 40 mg of pancreatic inhibitor were injected. Solid circles, control experiments, in which 6 units of insulin (40 units/kg) (no inhibitor) were injected.

9 MAY 1958

A systematic study of different trypsin inhibitors has revealed striking differences with respect to their susceptibility to peptic digestion (*2*) and to their ability to inhibit chymotrypsins (*8*). When these properties were taken into account, only colostrum inhibitor and pancreatic inhibitor were indicated for further study. Pancreatic inhibitor was more easily obtained and thus was used. Once-crystallized inhibitor was prepared according to the method of Kunitz and Northrop (*9*) from "fraction E" (*10*). The regular zinc insulin used was a commercial product (*11*).

Male Sprague-Dawley rats, weighing about 150 g each, were fasted overnight and were anesthetized with Pentothal (thiopental sodium, 40 mg/kg of body weight). The solutions to be investigated were mixed and injected into a loop of jejunum 20 cm long, ligated on both ends. Blood was obtained by clipping off the tip of the tail. Glucose content was determined by the Nelson-Somogyi method (*12*).

Ten experiments in which insulin and inhibitor were injected together were performed. In all ten, a significant drop in blood sugar was observed. Figure 1 illustrates the experiment in which the lowest, and Fig. 2, that in which the highest, dose was used. In other experiments, intermediate doses were used. Ten control experiments were performed by injecting insulin without inhibitor (Figs. 1 and 2); all results were negative. Two control experiments in which the inhibitor alone, and an additional experiment in which insulin plus an excess of protamine, was used, also gave negative results. None of the ten experimental animals died of insulin shock. The highest dose (Fig. 2) produced an effect approximately equivalent to 8 units/kg injected intraperitoneally, suggesting that, at the most, 3 percent of the injected insulin was absorbed.

Substitution of soybean inhibitor for pancreatic inhibitor, in amounts equivalent with respect to trypsin inhibiting power, resulted in very small and nonuniform responses. Since about 80 percent of each inhibitor remained in the loop after 4 hours of exposure, the difference cannot be ascribed to the instability of soybean inhibitor but suggests that pancreatic inhibitor partially protects insulin against destructive agents other than trypsin, whereas soybean inhibitor does not.

It had not yet been established that pancreatic inhibitor protected insulin from destruction. Inactivation in vivo occurred too fast for convenient measurements—that is, in the presence of 40 mg of inhibitor, of 35 units of insulin injected into the loop, only 5 percent was recovered after 3 minutes and less than 1 percent after 30 minutes; the absence of inhibitor did not influence the recovery of insulin after a short exposure, and barely a trace was recovered after 30 minutes. It was decided, therefore, to measure the rate of destruction of insulin in vitro,

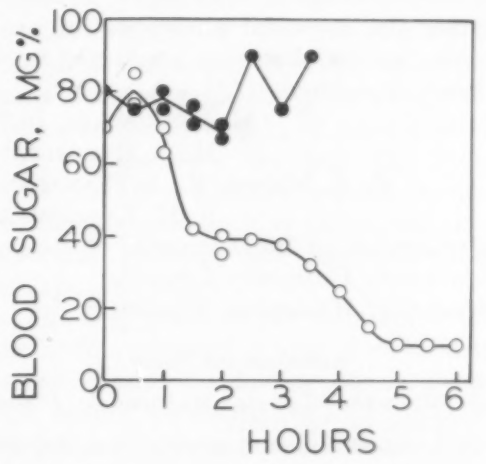

Fig. 2. Effect of intraintestinal administration of insulin on the blood sugar level. Open circles, experiments in which 35 units of insulin (250 units/kg) plus 100 mg of pancreatic inhibitor were injected. Solid circles, control experiments in which 35 units of insulin (250 units/kg) (no inhibitor) were injected.

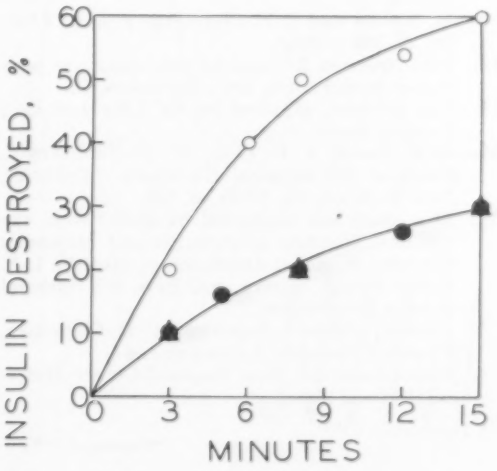

Fig. 3. Rate of destruction of insulin in vitro. Enzymes for the top curve (○) were obtained by injecting into a jejunal loop 1 ml of saline, allowing it to remain 10 minutes, excising the loop, and combining the contents with a 0.5-ml saline washing. Enzymes for the bottom curve (●) were obtained by the same procedure, except that saline containing 40 mg of pancreatic inhibitor per milliliter was used. The incubation mixture consisted of 0.4 ml of enzyme, 2.6 ml of saline containing 0.01*M* phosphate (*p*H 7.3), and 1 ml of insulin, 80 units/ml, at temperature of 37°C. At indicated times aliquots were withdrawn and diluted. In our control experiments, subcutaneous injection of 0.6 units/kg decreased the blood sugar level 35 to 45 percent, when the 1-, 2-, and 3-hour values were averaged and expressed as a percentage of the zero time value. Only dilutions of the in vitro enzyme-insulin mixtures which led to responses in this range were used to calculate percentage of inactivated insulin. Solid triangle, enzymes A to which pancreatic inhibitor was added before the addition of insulin.

and to slow the rate by dilution of the enzymes. The results are presented in Fig. 3 and show that inclusion of pancreatic inhibitor decreased the rate of insulin destruction.

The hypothesis that trypsin inhibitor is of physiological significance in facilitating the intestinal absorption of proteins (insulin) has been confirmed by a direct experiment (*13*).

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Iproniazid Treatment and Metabolism of Labeled Epinephrine in Schizophrenics

Previous work in this laboratory (*1, 2*) showed that when epinephrine labeled with carbon-14 in the beta position was infused into schizophrenic patients and normal subjects, essentially all of the radioactivity was recovered in the urine. When epinephrine labeled with carbon-14 in the methyl group of the side chain was infused, approximately one-third of the radioactivity was recovered in the urine. In both cases, the excretion of biologically active material represented only 1 to 5 percent of the infused epinephrine. A total of 14 infusions were performed. The urine of patients infused with beta-labeled epinephrine was selectively extracted and subjected to paper chromatographic analysis. A major radioactive metabolite was obtained, which possessed the solubility properties of a phenolic acid and had the same R_f values as authentic 3-methoxy-4-hydroxymandelic acid (*3–5*). This radioactive metabolite could not be demonstrated in the urine of patients infused with epinephrine labeled with carbon-14 in the methyl group of the side chain (*2*).

These data suggest the following hypotheses concerning the metabolic transformations of epinephrine: (i) The beta carbon atom remains attached to the benzene ring, and (ii) approximately two-thirds of the molecules of epinephrine lose the methyl group of the side chain. If one assumes that the methyl group of the side chain is lost, together with the amino group, under the influence of amine oxidase, then iproniazid treatment should result in more molecules of epinephrine retaining their methyl groups in the side chain. If this is the case, then more radioactivity should be recovered in the urine of patients receiving iproniazid and infused with methyl-labeled epinephrine.

Three female, chronic schizophrenic patients were placed on iproniazid, 100 mg/day, on 20 June 1957. The dosage was increased to 150 mg/day on 12 August. The first patient was infused with 0.5 mg of methyl-labeled *dl*-epinephrine on 3 September, the second on 18 September, and the third on 9 October. Fifty-nine, 74, and 63 percent of the infused radioactivity was recovered in the urine of these three patients, respectively. This is in contrast to 34 ± 3 percent recovered in the urine of four non-iproniazid-treated schizophrenic patients infused with the same amount of methyl-labeled *dl*-epinephrine. Both types of patients demonstrated typical cardiovascular responses to the infused epinephrine.

Two to 3 weeks after the cessation of iproniazid treatment, the first and second patients were again infused with methyl-labeled epinephrine. Fifty and 43 percent of the infused radioactivity was recovered in the urine of these two patients, respectively. This indicates that approximately half of the effect of iproniazid on monamine oxidase activity, as reflected by the metabolism of exogenously administered epinephrine, was still evident 2 to 3 weeks after the cessation of iproniazid therapy. Thus, approximately twice as many molecules of infused epinephrine retain the methyl group of the side chain when the patient is under iproniazid treatment in the dosages mentioned above as when he is not. These three patients varied in their psychiatric responses to iproniazid therapy. Nevertheless, all three patients showed a remarkably similar alteration in the metabolism of exogenously administered epinephrine.

The question then arose whether the increase in number of molecules retaining the methyl group following iproniazid treatment represents nondegraded, biologically active epinephrine or a stage in metabolism prior to amine oxidase action. Recently, Axelrod (*6*) reported the presence of methoxyepinephrine in the urine of rats, which was found in a greater amount following the intraperitoneal administration of iproniazid and epinephrine.

The following experiments were performed in our laboratory. The urine from patients was collected following the infusion of either beta-labeled or methyl-labeled epinephrine. The urine samples were lyophilized and stored at 0 to 5°C. The lyophilized urine was reconstituted with water and extracted for phenolic acids, according to the procedure of Armstrong *et al.* (*4*). The extracts were concentrated down to a small volume, *in vacuo*, at 45°C. An aliquot of the concentrated extract was chromatographed in the butanol : acetic acid : water system (4:1:5). Another aliquot was chromatographed in the two-phase solvent systems of Armstrong *et al.* (*4*). The phenolic acids were visualized by spraying with diazotized *p*-nitroaniline reagent. Autoradiograms were made from the chromatograms, in order to visualize these metabolites, which were derived from the infused labeled epinephrine. The urine which had been extracted for phenolic acids was hydrolyzed and selectively extracted for methoxyepinephrine in accordance with the procedures outlined by Axelrod (*6*). The extracts were concentrated down to a small volume *in vacuo*, at 45°C and subjected to paper chromatographic analysis, as outlined above.

The following results were obtained. The urine of non-iproniazid-treated patients infused with beta-labeled epinephrine consistently showed a major radioactive metabolite, which was a phenolic acid having the same R_f value as authentic 3-methoxy-4-hydroxymandelic acid. Very little methoxyepinephrine could be extracted from the urine of these patients. The urine of iproniazid-treated patients infused with methyl-labeled epinephrine consistently showed a major radioactive metabolite, which was a phenolic amine having the same R_f value as authentic methoxyepinephrine (*6*). The increase in excretion of radioactivity by the ipronized-treated patients infused with methyl-labeled epinephrine could be accounted for by the accumulation of methoxyepinephrine with a decrease in formation of 3-methoxy-4-hydroxymandelic acid.

The autoradiograms of urine obtained from patients infused with beta-labeled epinephrine showed the presence of another phenolic acid metabolite of epi-

nephrine. This metabolite occurred in very much smaller concentration than 3-methoxy-4-hydroxymandelic acid and has the following R_f values: isopropyl alcohol ammonia, 0.22; benzene propionic acid, 0.12. Authentic dihydroxymandelic acid (*3*) has R_f values of 0.25 and 0.19 in the afore-mentioned solvent systems. This radioactive metabolite, occurring in trace quantities, is tentatively considered to be 3,4-dihydroxymandelic acid.

The results of these experiments (*7*) clearly indicate that iproniazid treatment in man inhibits the action of monamine oxidase, but does not influence those enzymes which are responsible for the O-methylation of epinephrine.

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16 December 1957

Electrical Activity of Isolated Single Electroplax of Electric Eel as Affected by Temperature

In the last decade it has been shown that the permeability characteristics of the nerve membrane change during activity; the resistance decreases, and the electric currents propagating nerve impulses are carried by movements of Na^+ and K^+. Whereas there is little disagreement about this aspect, there are strongly opposing views about the mechanism by which these ion movements are controlled. Nachmansohn has persistently maintained the view that chemical processes must control this permeability change, and he and his associates have accumulated evidence that the acetylcholine system is inseparably associated with the elementary processes of nerve function—that is, the generation of bioelectric potentials (*1*). Support in favor of his views is the recent demonstration that lipid-soluble analogs of acetylcholine produce a depolarization of the active membrane (*2*).

On the other hand, purely physical processes are assumed by many leading physiologists to be responsible for the action potential; chemical reactions are considered to provide only the energy for restoring the ionic concentration gradients in the recovery period (*3*). The small initial heat production has been attributed to the mixing of Na^+ and K^+. Only a few measurements of temperature coefficients have been reported in the world literature (for reference, see *4*).

In view of the general interest in the problem whether or not the generation of bioelectric potentials requires chemical processes, and in view of the scarcity of data on temperature coefficients of conduction, we have evaluated the Q_{10} and the energy of activation over a wide range of temperatures on a recently developed preparation, the isolated single electroplax of the electric organ of *Electrophorus electricus* (*5, 6*). These organs are the most powerful electric generators created by nature, and they are highly specialized in their function; moreover, the preparation offers a favorable material for these studies. The duration of (i) the action potential, (ii) the latency period, and (iii) the postsynaptic potential has been studied as a function of temperature.

The duration of all three phenomena decreases with rise of temperature, whereas the amplitude of the spike and the postsynaptic potential remain unchanged (Figs. 1 and 2). Since there is a marked transitory change of permeability (*7*) during the action potential, the duration of the spike is a good measure of this change and pertinent for the question whether or not chemical reactions are involved in the process. If the logarithm of the reciprocal of the half-width of the spike is plotted against the reciprocal of the temperature according to Arrhenius, a straight line is obtained. This enables us to assign the energy of activation to the rate-controlling step in these processes.

The action potential elicited with direct stimulation has been studied at temperatures between 9° and 39°C. The Q_{10} has been found to be around 3.6, the energy of activation to be 21.000 cal/mole. The Q_{10}'s of the latency period and of the postsynaptic potential are very close to 2.6, and the energy of activation is around 16.000 cal/mole. An interesting observation in these experiments is the fact that it is impossible to elicit a postsynaptic potential and an indirect spike at temperatures above 32°C. This may indicate that the nerve action potential must have a certain duration above a critical level in order to be able to transmit the message across the synapse. The data support the conclusion that the three phenomena are dependent on chemical reactions. This conclusion is consistent with A. V. Hill's recent observation on the initial heat in nerve fibers (*8*). The latency period is frequently considered to be the result, partly at least, of the diffusion of a chemical transmitter from the tip of the axon to the postsynaptic membrane. Diffusion cannot have a Q_{10} of much greater than 1. Therefore, the high Q_{10} indicates that, if a diffusion process occurs, it is not the rate-limiting factor, but that chemical processes are responsible for the synaptic delay.

The Q_{10} found in the electroplax for the action potential is very close to that found in other conducting tissues. From the results published by Nastuk and Hodgkin (*9*), it is possible to calculate the Q_{10} for the duration of the action potential in the frog sartorius; its value is about 3.

The generation of bioelectric currents, the primary event in nerve conduction, is the only manifestation of living cells for which at present a purely physical

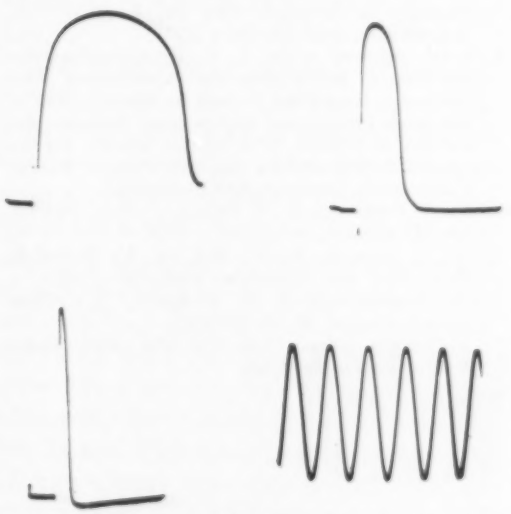

Fig. 1. Spike elicited by direct stimulation recorded with extracellular electrodes from a single isolated electroplax (*Electrophorus electricus*) at various temperatures. From upper left to lower right: 12°; 24°; 39°C; calibration, 50 mv, 2 msec.

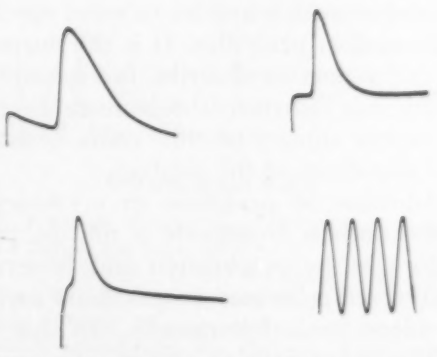

Fig. 2. Postsynaptic potential recorded with extracellular electrodes from a single isolated electroplax (*Electrophorus electricus*) at various temperatures. From upper left to lower right: 15°; 25°; 32°C; calibration, 5 mv, 1 msec.

process is offered as explanation and strongly supported by leading biologists. I consider the high values of the energy of activation reported here as incompatible with this view and as a support for those theories which postulate chemical processes as being responsible for the specific changes in permeability of conducting membranes during activity.

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30 January 1958

Circulating Antibody Directed against Penicillin

Detection, by in vitro serological techniques, of circulating antibodies directed against penicillin has not been reported. In the past few months, however, sera from certain individuals have been encountered which appear to react specifically against penicillin. It is the purpose of this report to describe the system in which this reaction is demonstrable and to report studies on the characteristics and specificity of the antibody.

Addition of penicillin to erythrocyte suspensions is frequently a routine procedure in the preparation and preservation of red cells used in specificity panels in blood-bank laboratories. In August 1957, during routine testing, the serum of a prospective transfusion recipient was found to agglutinate all of such a panel of erythrocytes prepared with penicillin; if the same erythrocytes were not exposed to penicillin, this serum caused no agglutination. Furthermore, the exposure of the patient's own erythrocytes to penicillin made them agglutinable by his own serum.

On further study it was found that human erythrocytes of all blood groups, by exposure to appropriate concentrations of penicillin G, O, or K, could be sensitized to react with this serum, and with sera of similar characteristics later discovered in other individuals. Once the erythrocytes were sensitized, they remained sensitized as long as they remained useful for testing—that is, until they began to show marked hemolysis—usually after at least 2 or 3 weeks. The sensitization was not affected by additional exposure of the sensitized cells to penicillinase for periods up to 4 hours, nor was it altered by exposure of the cells to 0.5-percent papain or 0.1-percent ficin.

Thus far, no human red cells have been shown to resist this "penicillinization."

Substitution of penicillinase, papain, or ficin for penicillin in the sensitization procedure gave negative results. Furthermore, in an attempt to see whether other antibiotics would sensitize erythrocytes for this reaction, approximately equal weights (about 10 mg) of the following antibiotics (*1*) were each dissolved in 1 ml of phosphate buffer (final *p*H 7.2 to 7.4) and then incubated wtih erythrocyte suspensions: streptomycin, dihydrostreptomycin, polymyxin B, bacitracin, neomycin B, ristocetin, viomycin, oleandomycin, synnematin B, and the penicillins G, O, and K. The only preparations which sensitized the red cells to react with the particular sera were the penicillins G, O, and K and synnematin B, which is another penicillin derivative.

Penicillin which had been inactivated by the addition of penicillinase (*2*) was no longer able to produce sensitization of erythrocytes.

Studies on the effects of varying the time of the exposure of the erythrocytes to varying concentrations of penicillin G were performed. It was found that the degree of sensitization of the red cells, as measured by their agglutinability by weakly reacting sera, varied directly with the time of exposure of the cells to penicillin and with the concentration of penicillin in the incubation mixture. For example, a 25-percent suspension of erythrocytes could be sensitized to approximately the same degree either by incubation for 24 hours with a concentration of penicillin of 3000 units/ml or by incubation for 10 minutes with a concentration of penicillin of 50,000 units/ml. Concentrations of penicillin of less than 3000 units/ml produced weak and irregular sensitization. Incubation times of more than 24 hours enhanced the sensitization to a slight or negligible degree. As a matter of convenience, therefore, the usual method of preparing "penicillinized" cells for the study of reactive sera has been to add about 8 ml of an equal-part mixture of whole blood and Alsever's solution directly to a vial containing 200,000 units of powdered penicillin G. After incubation at 37°C for 1 hour, an aliquot is withdrawn from the vial, the erythrocytes are thrice washed with isotonic saline and made up to a 4- to 10-percent suspension in saline. Since the sensitization proceeds at all temperatures from 6° to 37°C, the temperature of exposure does not seem to be critical.

Table 1. Inhibition of hemagglutination by prior addition of penicillin to reactive serum. (i) Penicillin + reactive serum = mixture; (ii) mixture + sensitized red blood cells → agglutination.

Concn. of penicillin added to reactive serum (units/ml)	Agglutination
0	2+
100	2+
370	2+
750	2+
1,500	+
3,000	±
6,000	±
12,000	0
25,000	0
50,000	0
100,000	0
200,000	0

Certain sera can be shown to react with erythrocytes prepared in such a fashion. With some sera the reaction can be demonstrated only by the antiglobulin technique. More strongly reacting sera, however, may agglutinate the sensitized erythrocytes directly from a saline suspension in a test tube, or even on a slide.

Sensitized cells exposed to these sera have been heated for 15 minutes at 54°C in saline to elute the antibody. The consequent eluate was demonstrated to react with other penicillinized erythrocytes.

This antibody is stable for at least several weeks at ordinary refrigeration temperatures and resists degradation by a temperature of 56°C for 2 hours.

Additional demonstration of the specificity of these sera was obtained by inhibition tests. In these, an attempt was made to see whether prior incubation of a reactive serum with penicillin would so bind the presumed antibody that the serum would no longer react with sensitized erythrocytes.

Solutions of penicillin G were made up in AB serum in concentrations from 100 to 200,000 units/ml. Equal volumes of each penicillin solution were added to

equal volumes of an appropriate serum on a slide. Penicillinized red cells were then added, and the mixtures were observed for agglutination. As may be seen in Table 1, the solutions containing the higher concentrations of penicillin completely inhibited the agglutination reaction.

Similar inhibition was demonstrated by means of the antiglobulin method. Equal volumes of a buffered solution of penicillin G and of a suitably diluted sample of an appropriate serum were incubated 15 minutes. This mixture was then tested against penicillinized cells by the antiglobulin technique. There was negligible agglutination in this tube, whereas in the tubes in which the buffer or AB serum was substituted for the solution of penicillin G, agglutination was marked.

Among approximately 2000 sera studied thus far, 25 have reacted specifically with "penicillinized" erythrocytes. All individuals from whom reactive sera were obtained have at some time in the past received penicillin therapy. Only a minority have demonstrated any clinical penicillin sensitivity. The significance of the antibody is currently being studied.

ALLYN B. LEY, JEAN P. HARRIS, MARY BRINKLEY, BOBBIE LILES, JAMES A. JACK, AMOS CAHAN
Department of Medicine and Blood Bank Laboratory, Memorial Center for Cancer and Allied Diseases, and *Knickerbocker Foundation, New York*

Notes

1. The antibiotics used in this study were kindly supplied by Dr. Henry Welch, Division of Antibiotics, Food and Drug Administration, Washington, D.C.
2. Penicillinase was kindly supplied by Dr. Bruno Puetzer, Schenley Laboratories.

11 December 1957

Color Coding of Stroboscopic Multiple-Image Photographs

The advantages of multiple-image photography for analyzing movement have been recognized ever since Marey developed "geometric chronophotography" in 1883 (*1*). The method has remained essentially the same, though in recent years it has been refined by the introduction of stroboscopic recording (*2*). A pattern for study is marked off on the subject in electric lights or reflecting material. The shutter of the camera is left open, and as the subject moves, the light source is interrupted at regular intervals. The movement is recorded as a time-space pattern on a single film. From the record, instantaneous displacements can be read directly, and velocities and accelerations can be obtained by differentiating.

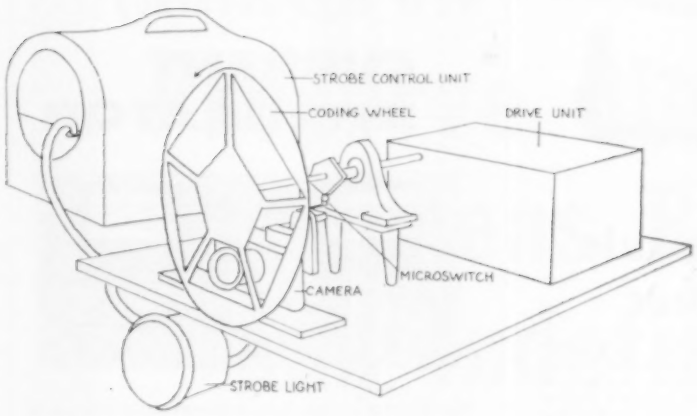


Fig. 1. Apparatus for color coding multiple-image photographs. Each aperture of the coding wheel is covered with a gelatine filter of a different color. As successive apertures come into place in front of the camera the microswitch is actuated, and a synchronized flash is emitted from the strobe.

Black-and-white photography is satisfactory for recording patterns that are relatively simple. As the patterns increase in complexity, however, interpretation becomes uncertain. The direction of movement is not given by black-and-white photography. (The movement pattern for standing-to-sitting, for example, cannot be distinguished by inspection from that for sitting-to-standing.) And when there is more than one trajectory in the pattern, it is sometimes impossible to determine which images are simultaneous. Neither problem is completely solved by the conventional method of omitting one flash from the cycle or altering its intensity.

Much of the confusion in a complex "stick pattern" can be eliminated by taking the photographs on color film and using a coding wheel to record successive images in different colors. If three or more colors are used in constructing the wheel, the direction of movement for each trajectory throughout the pattern will be recorded on the photograph. Simultaneity can readily be determined, since color automatically sorts out the images that belong together. With the help of color, the meaning of a complicated pattern can often be read at a glance. [Another type of coding may be obtained by varying the color of the reflecting material itself in order to distinguish one trajectory from another (*2*).]

We have used color coding as an aid in the analysis of human movement. A pattern for study is marked on the subject in Scotchlite reflecting tape (Silver No. 3270), and pictures are taken by coded flashes from a strobe unit. The apparatus is illustrated in Fig. 1.

The light source is a General Radio Strobolume with its lamp fastened directly below a Robot Star camera, which rests on a steel platform supported by a heavily built tripod. In front of the camera is a light aluminum wheel with a pentagonal center and five apertures, 5.5 cm at the greatest width. Each aperture is covered with a gelatine filter of a different color. A pentagonal cam on the shaft operates a microswitch so that, as successive apertures of the color wheel are centered in front of the camera, the Strobolume is actuated and a flash is emitted from the lamp. The wheel is driven by an 1800 rev/min synchronous motor at speeds of 1, 2, or 4 rev/sec, selected by a system of reduction gears. Pictures are taken on Ektachrome or Anscochrome film with an *f* stop of 4 when the camera is 12 feet from the subject.

The Strobolume operates at two intensities, high beam and low beam. The high beam, which has a flash duration of 40 μsec, cannot be operated for more than a few seconds at a time. The low beam has a flash duration of only 20 μsec and can be operated almost indefinitely at any of the rates we have used. So far, we have been able to record satisfactorily only with the high beam. With the faster color films now on the market it should ultimately be possible to record with the low beam. This would extend the range of movements that can be recorded by the method and add greatly to its usefulness.

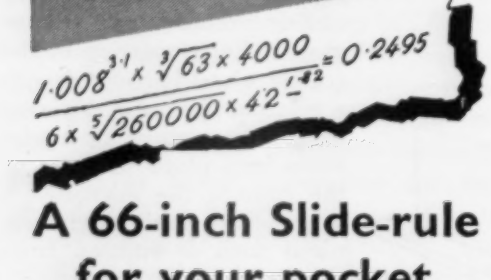
Once the apparatus has been set up, a transparency can be made with little trouble or expense. It provides a permanent movement-record that is easy to obtain, easy to interpret, and convenient to file (*3*).

FRANK PIERCE JONES
D. N. O'CONNELL
Institute for Applied Experimental Psychology, Tufts University, Medford, Massachusetts

References and Notes

1. E. J. Marey, *Compt. rend.* 96 (25 June 1883); *La Nature* (29 Sept. 1883).
2. F. P. Jones and D. N. O'Connell, *Phot. Sci. Tech. Ser. II* 3, No. 1, 11 (1956).
3. The method of color coding was developed under grants from the Carnegie Corporation of New York and the U.S. Public Health Service (R.G. 4836). An example of the method is reproduced in color in *Life* (17 Feb. 1958).

13 January 1958.



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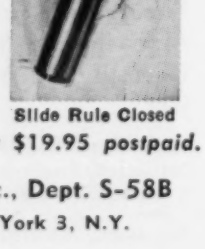

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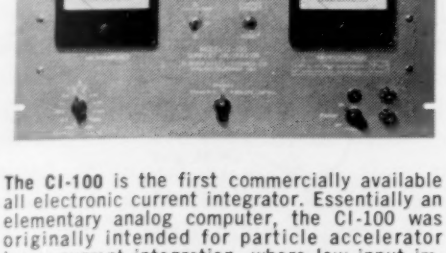

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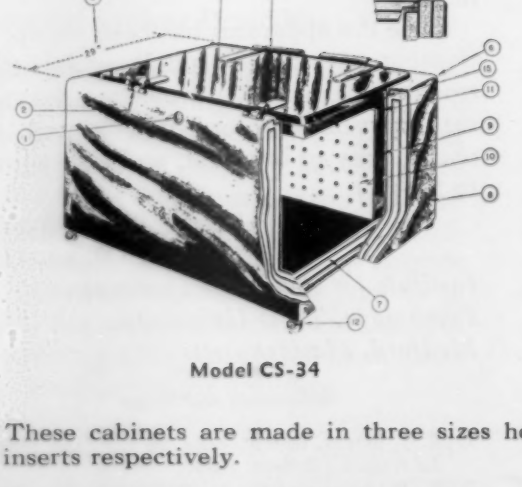

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Letters

Scientists and Government

The storm warnings are out, and all scientists should take heed. Recently, the "Parliament of Science," assembled by the AAAS in Washington, came out with several statements. One of these resolutions rejected a proposal for a federal Department of Science with cabinet status. The arguments marshaled were that the present arrangement has proved satisfactory in the past and in the present, and that the cabinet officer would necessarily be a political appointee and would be called upon to deal with basic research, which should not be amenable to political direction. Now on the surface these arguments appear to be irrefutable, but are they based on the facts of present-day scientific research? Are the scientists in this country today in the same position vis-à-vis the rest of society as they were even ten years ago?

The answer is no; and let me quote a revealing document. A report prepared by the staff of the Senate Government Operations Committee came out recently, dealing with an analysis of legislation to establish a Department of Science and Technology to coordinate the Government's scientific activities. This report accused the nation's scientists of evading responsibility in helping in this matter; further, "there does not appear to be much hope of obtaining objective, unbiased, and constructive recommendations from most of the scientists who normally would be called upon to assist the Congress in drafting a program for legislative action." And, "about the only alternative suggestion that has come to the attention of the staff has been that all basic science functions, whether Government controlled or supported, should continue to be placed under the exclusive administrative jurisdiction and control of scientists and the status quo maintained, except for increased funds." Well!

My contention is this. The nonscientific public, in and out of government, have become convinced in the last few years that any and all progress in military, economic, social, and even political matters can only be obtained with the massive aid of an organized, coordinated scientific program. We scientists may disagree, but there it is—these are the facts of scientific existence today. As a result, the role of the scientist has been changing, from one of being a virtual outcast from society to one of being forced to participate in the affairs of society whether he wants to or not. If this is the case, and I strongly believe it is, the next question arising is that of giving direction or control to scientific research. In this context, I believe that the men who drafted the proposal at the AAAS meet-

ing are living in a world which has ceased to exist. In this context, I believe that the report of the Senate committee is correct in that the scientists are evading their responsibility—responsibility not so much to government as to themselves and to other scientists.

And here we come to the crux of the matter; who is going to assume control and direction of scientific research in this country? The men assembled in the "Parliament of Science" are living in a dream world if they think that the old system concerning the government cornucopia is going to last much longer. At this moment, I would say that in general no one has control over the means and methods of research; this control has never been lost, it just hasn't existed. The existence of this lack of control is certainly not apparent from the deliberations of the "Parliament of Science"; conversely, it is all the Senate committee spoke about. It is clear to me that unless the so-called leaders and spokesmen of science cease their ostrich-like attitude concerning this matter, they and the rest of us scientists will find ourselves on the outside, working on projects whether we want to or not, over whose direction we have no say-so, and being subjected to the rules of nonscientific political officers, and having no opportunity to influence these men or the laws by which they govern.

My own feeling on what to do rests upon the assumption that only by going into political action, by working with political officers, and by trying to see to it that correct laws are passed and correct officials are instigated to do the correct things—correct by our scientific standards—can we avoid any resemblance to "Lysenkoism" in this country.

PHILIP SIEKEVITZ
Rockefeller Institute for Medical Research, New York, New York

Prepublication

With regard to your editorial of 21 March on prepublication, I believe that observance of a few very simple rules would eliminate any confusion in references to mimeographed material. This material appears in various forms but can probably be grouped into three general categories.

The first is, in essence, a personal letter that has been duplicated for multiple distribution. This is a private communication and should be referenced as such.

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Meetings

Experimental "Allergic" Encephalomyelitis

Since the development of Freund's ad juvants, much work has been done in ex perimental animals with nervous syste antigens in attempts to elucidate th etiology and pathogenesis of various neu rological disorders encountered in clin ical and veterinary medicine. Many di ferent disciplines have been involved- biochemistry, immunology, patholog microbiology, and so on—so that th published reports are widely scattere in the world literature, in journals a well as in books. Much information per taining to "allergic" encephalomyeliti has been published under titles whic might not be recognized by either in dexer or researcher as being related t this important experimental disease. In an attempt to assemble this large mas of pertinent data in a unified form, a symposium was held on "Experimenta 'Allergic' Encephalomyelitis and Its Re lation to Other Diseases of Man an Animals," 19 and 20 Oct. 1957, und the auspices of the National Advisor Council of the National Institute o Neurological Diseases and Blindnes Sixty scientists from many parts of th United States, Canada, England, Ger many, France, Italy, and Japan met a the National Institutes of Health, Be thesda, Maryland, to discuss histologi immunologic, and chemical aspects o these disorders. A brief summary of th data presented at this symposium ma be of interest.

Experimental "allergic" encephal myelitis can be produced in many spe cies by the injection of vaccines co taining brain and adjuvants, followi which various clinical neurological sig develop, especially paralysis. A perivs nous inflammation, often with demye nation, is seen histologically scattere through the central nervous system. Def nition of the experimental disease re quires consideration of (i) genetic an nutritional factors in the test animal (ii) the use of "priming" injections o suspensions of *Hemophilus pertussi* (iii) adjuvant factors and the route o inoculation; (iv) the chemical and im munologic nature of encephalitogeni materials isolated from neural or oth tissues; (v) local and general reactio produced in the test animal; and (vi) the nature of the reaction within th nervous system.

H. A. Schneider (New York) sum marized data obtained in mice which in dicate that susceptibility to the experi mental disease is inherited through a recessive gene. Susceptibility is influ enced by other factors, however, and can be abolished by feeding genetical susceptible mice a synthetic diet whi

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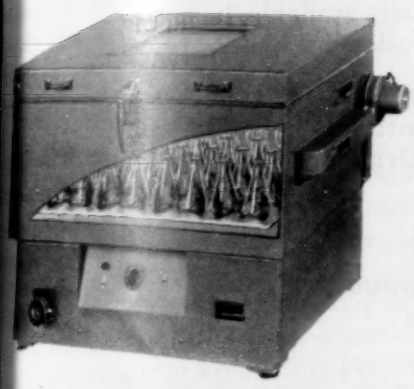

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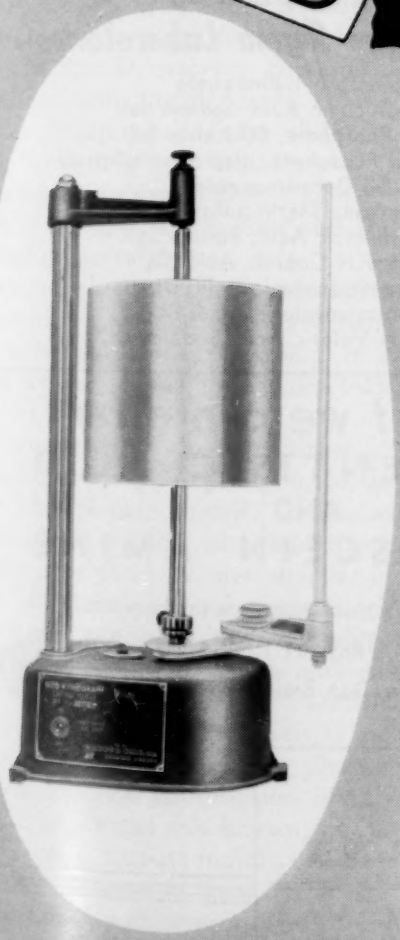

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adequate for maintenance of growth, especially if supplemented by Terramycin. Supplementation with vitamin B_{12}, folic acid, and biotin partially restores the susceptibility. Much further work must be done to define the crucial factors in the host and the environment, but it is possible that genetic and nutritional factors might account for the variable susceptibility of other species to different encephalitogenic materials.

Previous injection of suspensions of *B. pertussis* enhances the susceptibility of mice but has not been tried in other species. Its mechanism of action remains to be determined.

M. M. Lipton (Louisville, Ky.) reviewed the general mechanism of action of the components of Freund's adjuvants and concluded that the acid-fast bacilli and oil work together in the production of specific types of cells, directing the path of antibody formation toward cell-fixed as well as circulating types. The bacilli possibly also act as a *Schlepper*, making a hapten into a complete antigen. Of various routes, intradermal inoculation is generally considered most effective.

E. Lederer (Paris) correlated the immunologic and biologic effects of various lipids isolated from the tubercle bacillus: Tubercle formation is produced by many branched-chain fatty acids (including mycolic acid); delayed sensitivity to another substance is induced by carbohydrate esters of mycolic acid; and adjuvant activity is related to wax-D Canetti tripeptide with mycolic acid and polysaccharide). He attributed J. Colover's (Taplow, England) production of encephalitis with brain and a protein residue of tubercle bacilli to an insoluble polymer of wax-D, possibly present in the bacterial wall. Further work is necessary to determine if large doses of tubercle bacilli might be inhibitory or if an optimal ratio exists between concentrations of the bacillary factors and the encephalitogenic "antigens."

Three types of chemical substances obtained from nervous tissue have been found to be encephalitogenic, but it is still not possible to account for the total activity of whole brain:

1) Proteolipids, in doses of about 35 mg, have been found by J. Folch, M. B. Lees, B. H. Waksman, and R. D. Adams (Boston) to be capable of producing a relatively mild form of the disease in rabbits; others have found proteolipids to possess only minimal activity in guinea pigs [G. Clark (Buffalo); Kies and Alvord] and none in rats [P. Y. Paterson (New York)]. Most of this activity resides in the "ether-soluble lower phase." The exact chemical composition of this fraction is at present not known, but it is known that it consists mainly of lipids with a small amount of protein (4 per cent), presumably existing in proteolipid combination. J. M. Lee (New York) believes that proteolipid A, especially from homologous brain, is the only effective substance in mice.

2) Several proteins have been found by Kies, Roboz, and Alvord to be very effective in doses of 5 to 25 micrograms in guinea pigs, but others have found them not active in rats (Paterson), mice (Lee), and rabbits (Waksman). One of these proteins has been shown to be a single substance, by ultracentrifugation and electrophoresis, and to resemble a fragment of collagen in its high content of hydroxyproline and hydroxylysine (Roboz). Mild acid-extraction has yielded another protein-like encephalitogenic fraction from homologous guinea-pig brain (Kies).

3) A component has been isolated by Lipton from a petroleum ether extract of brain. In amounts of only a few micrograms it is effective in guinea pigs. Its chemical nature remains to be identified. Lipton believes it to be a phospholipid, but there is enough nitrogen to account for half of it being protein.

Immunologic data concerning these substances are still fragmentary. It is not known how they relate to the species-specific protein-like, and species-nonspecific lipid-like, neural antigens which E. Witebsky (Buffalo) has shown to be immunologically related but different in brain, spinal cord, posterior pituitary, and adrenal medulla. The last two organs are not encephalitogenic. Preliminary experiments reported by Witebsky, in which Coons's fluorescent-labeled antibody technique was used, suggest their localization in myelin sheaths. Waksman

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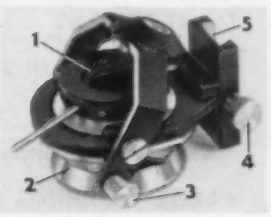

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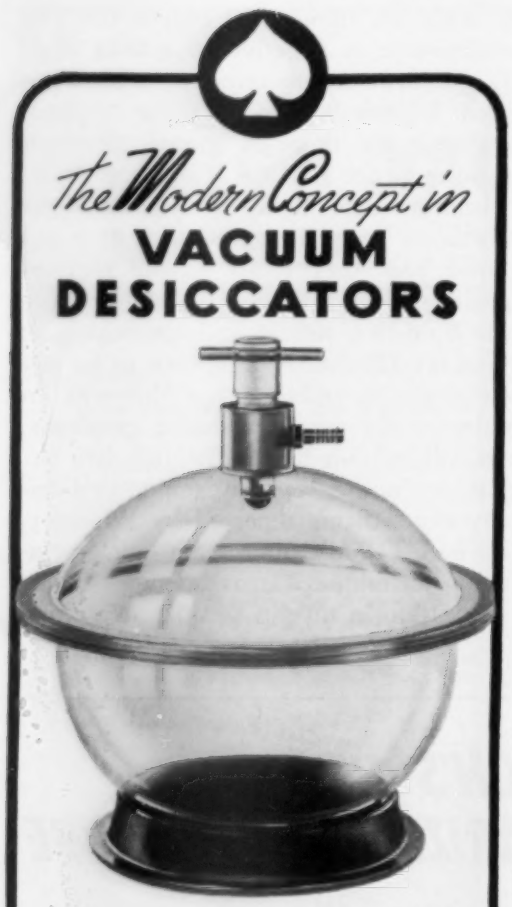

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reported delayed skin sensitization to homologous whole spinal cord or heterologous proteolipids paralleling the development of encephalomyelitis in rabbits, but M. W. Chase (New York) could not demonstrate such reactions to homologous brain in guinea pigs. M. Vulpé (Saskatoon, Canada) reported that a band appears between the alpha-1 and alpha-2 globulins in the electrophoretic pattern of serum of rabbits about 2 days before onset of paralysis, but whether this represents circulating antibody remains to be determined. Vulpé, A. Allegranza (Milan), and Murphy and Kies have failed to note any significant serum protein changes in guinea pigs during disease development.

B. Campbell and R. M. Condie (Minneapolis) emphasized the reaction of plasma cells not only locally and in the draining lymph nodes but also throughout the reticuloendothelial system and in the perivascular lesions in the central nervous system. In this last site, Adams and other neuropathologists insisted that adventitial histiocytes were also prominent. Correlation of the reaction of local epithelioid and distant cells with the various chemical fractions of the adjuvants and of brain remains to be made. Vogel's evidence on increased lipase activity in the regional lymph nodes has been extended by Vulpé and J. Olszewski, who found the degree of activity of this enzyme to be related to the lipid concentration of the material injected rather than to its encephalitogenic activity.

The site of the earliest lesion within the nervous system is still not determined: are the blood vessels (endothelium or adventitia) or the neural tissue (ground substance, glia, or myelin sheaths) primarily affected? Olszewski occasionally was able to demonstrate increased vascular permeability to radioactive iodinated serum albumin without leucocytic infiltration. L. Roizin (New York) has noted sudanophilic material in the endothelium, as well as marked accumulation of enzymes (oxidase, peroxidase, and phosphatase) with the perivascular cellular inflammation. He has also found periodic-acid-Schiff-positive, metachromatic material and sudanophilic, birefringent changes in myelin sheaths without inflammation.

The probable relation of the experimental disease to spontaneous neurologic diseases of man was discussed by H. Shiraki (Tokyo), who has studied the reaction occurring in human beings following injections of brain-containing vaccines used in the prevention of rabies. He described the differences between acute spinal and subacute or chronic cerebral forms of the reaction. Especially in the latter, large periventricular plaques strongly resembling multiple sclerosis plaques were noted.

The relation of experimental "allergic" encephalomyelitis to multiple sclerosis and other demyelinating diseases in man was further considered by Adams, who felt that the basic reaction was a perivenous necrobiosis related to white matter and that it was more consistent with the hypothesis of an allergic reaction than with any other hypothesis. J. G. Greenfield (London), E. W. Hurst (Macclesfield, England), W. Haymaker (Washington, D.C.), A. Wolf (New York), A. Ferraro (New York), K. H. Finley (San Francisco), and J. R. M. Innes (Tuckahoe, N.Y.) generally agreed with Adams, but H. M. Zimmerman (New York) insisted that the nervous system can react only in a limited number of ways to any noxious agent. F. C. Robbins (Cleveland) mentioned that Russian investigators had isolated a virus from cases of acute multiple sclerosis and were experimenting with a vaccine.

Certain diseases of other organs (peripheral nerves, adrenal, testis, thyroid, lens, uveal tract, and various components of blood) have also been thought to be due to an allergic reaction, more particularly to an autoimmunization. Colover described his work on the adrenal, and Witebsky summarized data on the thyroid which strongly suggest that at least certain cases of chronic thyroiditis in man are due to the development of antibodies against the individual's own thyroglobulin. The only missing steps in the proof of this hypothesis are the production of thyroiditis by passive transfer of antibodies and the determination of the "trigger" mechanisms for release of thyroglobulin in man.

In discussing the pathogenesis of postinfectious encephalomyelitis, Robbins said he felt that the primary virus or another virus (latent or simultaneously infecting the patient) might directly invade the nervous system, break down the blood-brain barrier, and damage myelin or some other element of the nervous system. If the breakdown product then gets into the blood stream and becomes antigenic, the antibodies might in turn cause further damage in nervous tissue. Although W. S. Wood (Chicago) has found skin-sensitization to rabies vaccine in cases of rabies postvaccinal encephalomyelitis, he has not found it in cases of postinfectious encephalomyelitis.

Although all the available evidence concerning experimental "allergic" encephalomyelitis is consistent with the theory of allergy, it must be pointed out that at least two critical steps remain to be demonstrated: (i) the acceleration of the disease by previous specific sensitization, and (ii) the production of the disease by passive transfer of specific antibodies. Only negative or equivocal evidence for the latter has so far been obtained (Chase, Waksman, Vulpé, Wood, and Condie), and the available evidence concerning the former indicates that protection rather than acceleration

afforded by prior injections of incomplete vaccines containing brain or adjuvants alone (Kies *et al.*, Paterson, Condie, Waksman, Zeman, and Ferraro). Although negative, these observations suggest that allergy may not play an important role in the development of the experimental disease and indicate that further work is necessary to settle an important question: is allergy a significant factor in diseases of the nervous system?

ELLSWORTH C. ALVORD, JR.
Baylor University College of Medicine, Houston, Texas
MARIAN W. KIES
Laboratory of Clinical Science, National Institute of Mental Health, Bethesda, Maryland

Forthcoming Events

June

9–11. American Assoc. of Spectrographers, 9th annual symp., Chicago, Ill. (H. J. Hettel, Armour Research Foundation, 10 W. 35 St., Chicago 16.)

9–11. Canadian Federation of Biological Societies, 1st annual; with Canadian Assoc. of Anatomists, Canadian Biochemical Soc., Canadian Physiological Soc., and Pharmacological Soc. of Canada; Kingston, Ontario. (E. H. Bensley, Montreal General Hospital, 1650 Cedar Ave., Montreal 25, P.Q.)

9–11. Health Physics Soc., 3rd annual, Berkeley, Calif. (E. E. Anderson, Oak Ridge National Lab., Oak Ridge, Tenn.)

9–11. Soc. of General Physiologists, Woods Hole, Mass. (F. G. Sherman, Dept. of Biology, Brown Univ., Providence 12, R.I.)

9–11. Society for the Study of Development and Growth, 17th annual symp., South Hadley, Mass. (Miss K. Stein, Dept. of Zoology, Mount Holyoke College, South Hadley.)

9–12. Microscopy Symposium, 5th, Chicago, Ill. (W. C. McCrone, Jr., 500 E. 33 St., Chicago 16.)

9–13. Automation Exposition and Cong., 4th Internatl., New York. (International Automation Exposition, c/o Richard Rimbach Assoc., 845 Ridge Ave., Pittsburgh 12, Pa.)

10–12. Astronomical Soc. of the Pacific, annual, Los Angeles, Calif. (S. Einarsson, Leuschner Observatory, Univ. of California, Berkeley 4.)

10–13. Vacuum Techniques, 1st internatl. congress, Namur, Belgium. (E. Thomas, c/o CSN/ERM, 30, avenue de la Renaissance, Brussels 4, Belgium.)

11–14. Applied Mechanics, 3rd natl. Cong., Providence, R.I. (W. Prager, Brown Univ., Providence 12.)

11–14. National Soc. of Professional Engineers, St. Louis, Mo. (P. H. Robbins, NSPE, 2029 K St., NW, Washington, D.C.)

14–21. American Soc. of Medical Technologists, annual, Milwaukee, Wis. (Miss R. Matthaei, Suite 25, Hermann Professional Bldg., Houston 25, Tex.)

15–19. American Soc. of Mechanical

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Engineers, semiannual, Detroit, Mich. (O. B. Schier, II, ASME, 29 W. 39 St., New York 18.)

15–19. Cancer Research Conf., 3rd Canadian, Honey Harbour, Ontario. (R. L. Noble, Collip Medical Research Lab., Univ. of Western Ontario, London, Ont., Canada)

15–20. American Physical Therapy Assoc., annual, Seattle, Wash. (Miss M. E. Haskell, APTA, 1790 Broadway, New York 19.)

16–18. American Neurological Assoc., 83rd annual, Atlantic City, N.J. (C. Rupp, 133 S. 36 St., Philadelphia 4, Pa.)

16–18. Military Electronics Conv., 2nd, Washington, D.C. (G. Rappaport, Emerson Radio & Phonograph Corp., 1140 East-West Highway, Silver Spring, Md.)

16–18. Photochemical Apparatus Symp., Upton, N.Y. (R. C. Fuller, Biology Dept., Brookhaven National Laboratory, Upton, L.I.)

16–20. American Soc. for Engineering Education, annual, Berkeley, Calif. (W. L. Collins, Univ. of Illinois, Urbana.)

16–20. Association of Official Seed Analysts, annual, Montreal, Quebec, Canada. (L. C. Shenberger, Seed Lab., Dept. of Agricultural Chemistry, Purdue Univ., Lafayette, Ind.)

16–20. Molecular Structure and Spectroscopy Symp., Columbus, Ohio. (R. A. Oetjen, Dept. of Physics and Astronomy, Ohio State Univ., Columbus 10.)

16–20. Pacific Div., AAAS, annual, Logan, Utah. (R. C. Miller, California Acad. of Sciences, Golden Gate Park, San Francisco 18.)

17–19. American Dairy Science Assoc., annual, Raleigh, N.C. (H. F. Judkins, 32 Ridgeway Circle, White Plains, N.Y.)

17–19. American Meteorological Soc., with Pacific Div., AAAS, Logan, Utah. (K. C. Spengler, AMS, 3 Joy St., Boston 8, Mass.)

18–20. Statistical Methods in Radio Wave Propagation, intern. symp., Los Angeles, Calif. (W. C. Hoffman, 3116 Engineering Bldg., Univ. of California, Los Angeles 24.)

18–21. College Physicists, 20th annual colloquium, Iowa City, Iowa. (J. A. Van Allen, Dept. of Physics, State Univ. of Iowa, Iowa City.)

18–22. American College of Chest Physicians, annual, San Francisco, Calif. (M. Kornfeld, ACCP, 112 E. Chestnut St., Chicago 11, Ill.)

19–21. Endocrine Soc., 40th annual, San Francisco, Calif. (H. H. Turner, 1200 N. Walker St., Oklahoma City 3, Okla.)

19–21. Society of Nuclear Medicine, 5th annual, Los Angeles, Calif. (R. W. Lackey, 452 Metropolitan Bldg., Denver, Colo.)

19–25. Scandinavian-American Meteorological Meeting, Bergen, Norway. (K. C. Spengler, 3 Joy St., Boston, Mass.)

21–22. Society for Investigative Dermatology, annual, San Francisco, Calif. (H. Beerman, 255 S. 17 St., Philadelphia 3, Pa.)

22–25. American Soc. of Agricultural Engineers, 51st annual, Santa Barbara, Calif. (J. L. Butt, ASAE, St. Joseph, Mich.)

22–25. Medicinal Chemistry, 6th natl. symp., Madison, Wis. (E. Smissman, College of Pharmacy, Univ. of Wisconsin, Madison.)

22–27. American Inst. of Chemical Engineers, 50th anniversary, Philadelphia, Pa. (F. J. Van Antwerpen, AIChE, 25 W. 45 St., New York 36.)

22–27. American Soc. for Testing Materials, 61st annual, Boston, Mass. (F. P. Van Atta, ASTM, 1916 Race St., Philadelphia 3, Pa.)

23–24. Unstable Chemical Species Symp., Los Angeles, Calif. (Directorate of Advanced Studies, Air Force Office of Scientific Research, P. O. Box 2035-D, Pasadena, Calif.)

23–25. American Soc. of Heating and Air-Conditioning Engineers, semiannual, Minneapolis, Minn. (A. V. Hutchinson, ASHAE, 62 Worth St., New York 13.)

23–25. American Soc. of Refrigerating Engineers, annual, Minneapolis, Minn. (R. C. Cross, ASRE, 234 Fifth Ave., New York 1.)

23–27. American Soc. of Civil Engineers, Portland, Ore. (W. H. Wisely, ASCE, 33 W. 39 St., New York 18.)

23–28. Low Temperature Physics, 6th internatl. conf., Leiden, Netherlands. (J. van den Handel, Kamerlingh Onnes Laboratory, Leiden.)

(See issue of 18 April for comprehensive list)

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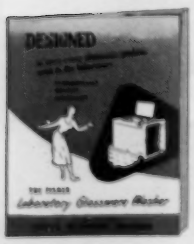

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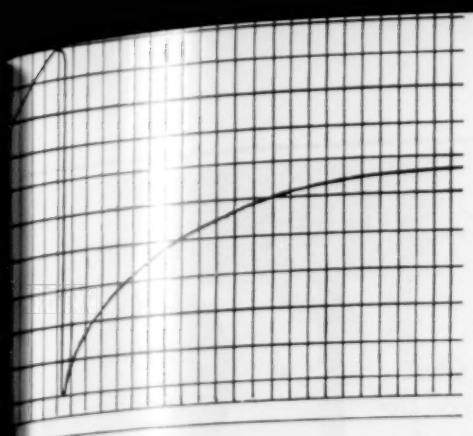

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■ BIOCHEMICALS, more than 2100 in number, are listed with prices in a 1958 catalog. Many new listings are included. (Nutritional Biochemicals Corp., Dept. 29)

■ NOISE TUBE, type TD-22, is designed for use as a noise source in super-high-frequency measurements. It is constructed for use in Rg/48U waveguide to provide noise in the 7.6- to 11.5-cm band (Bendix Aviation Corp., Dept. 30)

■ MICRO-DISTRIBUTION SAMPLER assists the microscopist in volumetric analysis of microscopic specimens. The instrument includes an electrically operated mechanical stage which scans the specimen in discrete predetermined steps. The operator identifies the constituent under the cross-lines and presses one of either 7 or 14 keys which record and count the occurrences of constituents at the sample points. The result is a recorded distribution of up to 14 constituents in the sampling grid pattern. (Hilger and Watts, Dept. 31)

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■ HIGH-VACUUM PUMPING STATION provides high-speed pump-down and ultimate vacuum of less than 10^{-8} mm-Hg. The cold trap operates from 8 to 10 hr on one filling. The ion gage used for vacuum indication to 2×10^{-9} mm-Hg is of non-burnout design. Drift is less than 2 percent per day. The Chemstone top of the equipment can be used as a work table. (Veeco Vacuum Corp., Dept. 37)

■ ENVIRONMENTAL CHAMBER of 16-ft³ capacity offers a temperature range from ambient to −130°F. The chamber is designed to be explosion-proof, air circulation is accomplished by an externally mounted explosion-proof motor. Thermal capacity is 5800 Btu/hr at −100°F. Interior is made of 14-gage stainless steel. Access is at top through a counterbalanced lid. (Webber Corporation, Dept. 48)

■ POINT LIGHT SOURCE is a compact optical system which concentrates the light output of a high-pressure mercury lamp into a small-diameter point of high intensity. Source diameters range from 0.002 to 0.16 in. with corresponding intensities ranging from 3 to 300 ca. Beam width is a full 180-deg hemisphere. The 100-w mercury lamp is housed in a metal case. The optical system comprises a condenser lens, objective lens, demagnifier and dispersal lens, and appropriate apertures and filters. Adjustable lenses may be provided to produce varied point characteristics within limits. Small fans cool the lenses. (De Florey Co., Dept. 43)

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